
SCIENCE

FRIDAY, APRIL 10, 1896.

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MSS. intended for publication and books etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE NATIONAL ACADEMY OF SCIENCES AND THE COLLEGES OF THE UNITED STATES.

THE National Academy of Sciences was incorporated by an Act of Congress in the year 1863. It consists of 88 members at present, and adds to its numbers annually seldom more than five members, and often none.

The following interesting table shows how its members are distributed among the faculties of the various Colleges in the United States, according to the data available in the Lick Observatory. As our set of college catalogues is not complete a few errors may remain in this list. Professor Hilgard has been kind enough to revise the table before printing. Names in square brackets belong, officially, in another category also.

Albany: Museum of the State of New York, (1): J. Hall.

Albany: Union College (The Dudley Observatory), (1): L. Boss.

Baltimore: Johns Hopkins University, (4): W. K. Brooks, I. Remsen, H. A. Rowland, [S. Newcomb] W. H. Welch.

Berkeley: The University of California, (4): G. Davidson, E. W. Hilgard, E. S. Holden, Joseph Le Conte.

Boston: The Society of Natural History, (1): Alphæus Hyatt.

Boston: The Mass. Inst. Technology, (2): J. M. Crafts, F. A. Walker.

Cambridge: Harvard University, (11): A. Agassiz, H. P. Bowditch, W. G. Farlow,

W. Gibbs, G. L. Goodale, H. B. Hill, C. L. Jackson, E. C. Pickering, F. W. Putnam, C. S. Sargent, J. Trowbridge.

Chicago: The University of Chicago, (1): A. A. Michelson, C. O. Whitman.

College Hill: Tufts College, (1): A. Michael.

Hoboken: Stevens Technological Institute, (2): A. M. Mayer; H. Morton.

New Haven: Yale University, (13): W. H. Brewer, G. J. Brush, R. H. Chittenden, E. S. Dana, W. L. Elkin, J. W. Gibbs, C. S. Hastings, S. W. Johnson, O. C. Marsh, H. A. Newton, S. I. Smith, A. E. Verrill, A. W. Wright.

New York: American Museum, (1): J. A. Allen.

New York: Columbia College, (3): C. F. Chandler [G. W. Hill], R. Mayo-Smith, O. N. Rood.

New York: The Public Library (—): [J. S. Billings].

Philadelphia: University of Pennsylvania, (4): G. F. Barker [J. S. Billings], E. D. Cope, J. P. Lesley, H. C. Wood.

Princeton: College of New Jersey (1): C. A. Young.

Providence: Brown University (2): C. Barus, A. S. Packard.

Washington: U. S. Army, (5): H. L. Abbot, J. S. Billings, C. B. Comstock, E. Coues, C. E. Dutton.

Washington: American Ephemeris, (—): [G. W. Hill], [S. Newcomb].

Washington: U. S. Navy, (2): A. Hall, S. Newcomb.

Washington: U. S. Coast and Geodetic Survey, (1): C. A. Schott [H. Mitchell].

Washington: U. S. Geological Survey (5): G. F. Emmons, G. K. Gilbert, A. Hague, R. Pumpelly, C. A. White.

Washington: U. S. Weather Bureau, (1): C. Abbe.

Washington: Smithsonian Institution and National Museum and Fish Commission, (4): T. N. Gill, G. B. Goode, J. P. Langley, J. W. Powell.

Waterville: Colby University, (1): W. A. Rogers.

Worcester: Polytechnic Institute, (1): T. C. Mendenhall.

In Private Life: (15): A. G. Bell, S. C. Chandler, B. A. Gould, G. W. Hill, C. King, M. C. Lea, T. Lyman, H. Mitchell, S. W. Mitchell, E. S. Morse, C. S. Pierce, F. Rogers, S. H. Scudder, W. Sellers, J. H. Trumbull.

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DIFFUSIVE REFLECTION OF RÖNTGEN RAYS.*

THE following communication contains a brief description of a series of experiments with Röntgen radiance which were conducted during the last six weeks. The results of these experiments seem to possess a sufficient scientific and practical importance to merit notice. The most important refer to diffuse reflection or scattering of Röntgen radiance. It seems desirable to state first, briefly, the disposition of the apparatus and the method of experimentation by means of which the Röntgen effects can be rendered sufficiently intense for the purpose described below.

Induction Coil and Interrupter. A powerful coil was found indispensable for strong effects and satisfactory work. The vibrating interrupter is too slow and otherwise unsatisfactory, and it was replaced by a rotary interrupter, consisting of a brass pulley, 6 inches in diameter and $1\frac{1}{4}$ inches in thickness. A slab of slate $\frac{3}{4}$ inch thick was inserted and the circumference was kept carefully polished. This pulley was mounted on the shaft of a Crocker-Wheeler $\frac{1}{8}$ HP motor giving 30 revolutions, and, therefore, 60 breaks per second. Two adjustable Marshall condensers of three microfarads each were connected in shunt with the break,

* Presented before the New York Academy of Sciences, April 6, 1896.

and the capacity adjusted carefully until the break-spark was a minimum and gave a sharp cracking sound. Too much capacity will not necessarily increase the sparking, but it will diminish the inductive effect which is noticed immediately in the diminished intensity of the discharge. A powerful coil with a smoothly working rotary interrupter will be found a most satisfactory apparatus in experiments with Röntgen radiance.

Edison's Fluoroscope. A fluorescent screen, made by Aylsworth & Jackson, of East Orange, N. J., according to Mr. Edison's directions, will be found an indispensable aid in these experiments. The salt employed is tungstate of calcium and it is so powerful that with a satisfactorily working tube it will show a noticeable fluorescence at a distance of *over thirty feet.* Those who have struggled with barium-platino-cyanide screens will appreciate fully Mr. Edison's improvement. This fluorescent screen was employed successfully for three distinct purposes. First, to study the operation of the vacuum tube under various conditions; secondly, to shorten the time of exposure in photography; and thirdly, to study the phenomena of diffuse reflection.

The most Efficient Working of the Tube—The Critical Temperature. The tubes employed were an old pear-shaped Crookes tube with a cross and several pear-shaped German tubes, imported sometime ago by Eimer & Amend, of New York. They all had discs at each electrode. Very satisfactory tubes are also being made now at the lamp works of the General Electric Company at Harrison, New Jersey. These were also employed in my experiments with completely satisfactory results. No fresh tube works quite satisfactorily with a powerful coil and a rapid rate of interruption; it heats too much, and the vacuum becomes thus rapidly impaired and the intensity of the Röntgen radiance is very much diminished. This is true even of larger tubes. Each new tube must undergo first an electric treatment. I have described this matter at some length at the meeting of the Academy on March 2d. Since that time I have investigated it more fully and brought it to a satisfactory termination. Mr. Tesla has also in the meantime discussed this matter, but in what appears to me to be a somewhat fanciful way. He imagines that the vacuum of a Crookes tube becomes more and more attenuated by the passage of current through it on account of the expulsion of the gas through the walls of the bulb. He maintains that he even succeeded in piercing electrically a small hole in the tube through which the gas from the vacuum was expelled with so enormous a velocity as to prevent the outside air from rushing in. This marvelous experiment does certainly support Mr. Tesla's favorite molecular bombardment theory, but it seems to leave us with the gloomy propect of having to refill our tubes from time to time with a fresh vacuum. The following experiments, however, lead to the conclusion that this necessity will probably never arise and that Mr. Tesla's interpretation of the cause of variation of the vacuum during the discharge is probably wrong. The electrical treatment of the tube is simply this: Pass a sufficiently strong current until the tube becomes so hot as to lose much of its Röntgen radiance. Stop then and let it cool. Repeat the operation and observe that after each operation the vacuum has gone up and that the Röntgen effect becomes stronger. It is not advisable to drive the vacuum much beyond the sparking distance between the electrodes on the outside. But even if this point has been reached then a judicious application of the Bunsen flame to the tube will enable the coil to force and maintain a strong enough current through the tube so as to heat it gradually, which increases the facil-

ity with which the discharge passes through the tube. The fluoroscope tells us that there is then a perfectly definite temperature at which the tube will work most efficiently and it is desirable to operate the tube at this temperature. This can be easily done by directing currents of air against those parts of the tube where it heats most, that is, against the parts opposite to the electrodes. By a suitable regulation of the air currents and a careful watching with the fluoroscope the tube can be kept steadily at the temperature of its highest efficiency for hours. A deviation above or below this point will produce a very large diminution in the Röntgen effect. This temperature of highest efficiency is so sharply defined that it looks very much like a critical point in the discharge. Below this temperature the discharge is in straight lines from the cathode and the portion of the glass opposite to the cathode fluoresces much more intensely than the rest. Above this temperature the discharge begins to spread in all directions from the cathode and the whole tube fluoresces strongly. There is then considerable flickering until the temperature is sufficiently above the critical temperature. The tube heats then rapidly and a yellowish mist begins to rise from the anode. As soon as the air blast begins to cool the tube this mist begins to clear away and the whole tube regains a clear transparency. If the tube is made too cool the discharge becomes too faint; there is very little heating because the coil fails to force a strong enough current. The Bunsen burner will assist the coil then to force a sufficiently powerful discharge again. The blackening of the tube by the disintegration of the electrodes seems to be the only thing that will determine the length of its life.

The Combination of a Fluorescent Screen with a Photographic Plate. Photography at a Long Distance from the Tube and through the Heavier Parts of the Human Body.—With an arrangement of apparatus as described above it was found possible to produce very much stronger photographic effects, but not sufficiently strong for penetration through the thigh and the trunk of the human body at reasonably short exposures and at long enough distances from the tube to obtain the desirable clearness in the pictures of these massive parts. A completely successful application of Röntgen's beautiful discovery to surgery depends for the present on a successful solution of the problem just mentioned. I have obtained one satisfactory solution with the method which I first described before the Academy on March 2d. It consists in placing in contact with the photographic plate a fluorescent screen and thus transforming most of the Röntgen radiance into visible light before it reaches the sensitive film. Photographs of the hand were thus obtained at a distance of twenty-five feet from the tube with an exposure of half an hour. At the distance of four inches the hand can be photographed by an exposure of a few seconds. It was in this manner only that I succeeded in photographing on a single plate the whole chest, shoulders and neck of my assistant, with an exposure of seventy minutes and at a distance of three feet between the plate and the tube. The collar button and the buttons and clasps of the trousers and the vest show very strongly through the ribs and the spinal column. This result seems to prove beyond all reasonable doubt the applicability of radiography to a much larger field in surgery than was expected a few weeks ago.

Diffuse Reflection of the Röntgen Radiance.—The question of reflection and refraction of the X-rays is a very important one. It was discussed by Prof. Röntgen in sections 7 and 8 of his original essay. Neither by photography nor by the fluorescent screen could he detect an appreciable refraction

with certainty. A reflection from metallic surfaces in the immediate vicinity of a photographic film was detected, "but," quoting Röntgen's own words, "if we connect these facts with the observation that powders are quite as transparent as solid bodies and that, moreover, bodies with rough surfaces are in regard to the transmission of X-rays, as well as in the experiment just described, the same as polished bodies, one comes to the conclusion that regular reflection, as already stated, does not exist, but that the bodies behave to the X-rays as muddy media do to light."

In face of these observations made by Prof. Röntgen, Prof. Rood's and Mr. Tesla's experiments must be interpreted as a confirmation of Prof. Röntgen's results, and not as a demonstration of the existence of a regular reflection. If I understand Prof. Rood's words correctly, no claim is made by him of a discovery of regular reflection; for he says: "These facts and the character of the deformations point very strongly to the conclusion that in the act of reflection from metallic surface the Röntgen rays behave like ordinary light." Mr. Tesla, however, infers with much confidence regular reflection from his theory of bombardment. His experimental method is the same as that of Prof. Rood; that is, he places a reflecting plate at an angle of forty-five degrees to the direct ray and then places the photographic plate at right angles to the direction in which the reflected ray should pass if regular reflection existed. On account of the greater power of his apparatus, his time of exposure was one hour, whereas that of Prof. Rood was ten hours. It is evident, however, that an effect upon the photographic plate does not prove the existence of regular reflection, as Mr. Tesla maintains with much assurance and with much rejoicing over the realization of the prophesy which he made, inspired by his molecular bombardment theory.

In my experiments on reflection I aimed at getting rid of the photographic plate and substituting the fluorescent screen in its place. Two conditions had to be fulfilled to make this substitution possible. First, a very powerful and perfectly steady discharge had to be maintained. Secondly, a very sensitive fluoroscope had to be employed. The first was accomplished by the apparatus and the operations described above. The second was found in Mr. Edison's tungstate of calcium fluoroscope. The tube was placed between two thick planks of pine coated with sheet lead $\frac{1}{16}$ of an inch thick. This screening was found to be somewhat insufficient when the tube operated at maximum efficiency and another screen consisting of a thick copper plate had to be employed. The planks were placed so as to form a wedge around the tube. The cathode streamer was horizontal and passed through a vertical slit formed by the edges of the two screening lead-covered planks. In front of this slit was a fixed pivot on which a mirror could be rotated. The mirror consisted of a polished sheet of platinum pasted upon a rectangular piece of pine board of nearly the same area as the platinum sheet and about one inch thick. The slit was made $\frac{1}{16}$ in. wide and its image examined was by means of the fluoroscope. The tube was six inches from the slit.

a. Quite near the slit the image was sharp and intense. But as the fluoroscope was gradually moved away from the slit its image broadened out somewhat, and there was at each side of it a diffuse border. At about two inches from the slit the image of the slit looked like a wide spectral line upon the less luminous background of a wide band which shaded off gradually into the dark space of the screen. With increase of distance the relative intensities of the two grew more and more equal, and at about six inches from the slit the whole fluorescent

screen (about 6 inches by 4 inches) was uniformly illuminated. There was evidently a diffuse scattering of the X-rays in their passage through the air. This inference was confirmed by other experiments which will be discussed presently. Various well-known devices were employed to concentrate the cathode rays along the axis of the tube. So, for instance, wrapping tinfoil around the tube. This, however, did not diminish the gradual diffusion of the image of the slit on the fluorescent screen when the distance between the slit and the fluoroscope was gradually increased. Up to about three inches from the slit the real image of the slit could still be distinguished easily from the diffuse background as a band of maximum intensity.

b. The platinum mirror was now placed quite near the slit and at a convenient angle to the direction of the ray, and the fluoroscope was placed quite near the mirror. There was a faint illumination of the fluorescent screen, but it was perfectly uniform. Not the slightest indication of an image of the slit could be detected, although the distance between the slit and the mirror plus the distance between the mirror and the fluorescent screen was less than the distance at which the image of the slit on the fluorescent screen appeared as a band of maximum intensity when observed directly. A change in the angle of the mirror produced but a small change in the fluorescence of the screen, and then the change seemed to be such as to approach a maximum when the mirror and the fluorescent screen were parallel to each other. The same experiment was repeated with other metals and with the same result. This experiment, therefore, does not speak in favor of regular reflection.

c. Turning the mirror completely around, so that the face of the wooden block on which the metal plate was fastened served as a mirror it was found that the fluorescent effect upon the screen was stronger than with the platinum. A pad of paper of about the same size as the wooden block acted more strongly than the platinum or any other metal. Various substances were tried, like glass, vulcanite, the hand, various metals, and they all produced a diffuse reflection of varying intensity, and at all angles of inclination. In all cases the maximum effect seemed to take place when the broadest side of the reflecting object was about parallel to the fluorescent screen. But the fluorescence was very weak as long as the slit was narrow.

d. The slit was now made wider, and the same series of experiments were repeated with various widths of the slit. The fluorescence of the exploring screen increases, of course, with the width of the slit. The observations made with the narrow slit were confirmed. In every case the maximum intensity on the exploring screen was obtained when the broadest side of the reflecting object was about parallel to the screen. Wood and transparent insulators produced a stronger effect than metals. No accurate quantitative comparisons have yet been made. Among the insulators experimented with, wood produced the strongest effect, and among the metals aluminium is the weakest for the same thickness of the plate. The thickness of the reflecting plate increases the effect; this increase will go on until the reflecting plate is several inches thick if this plate is an insulator. In the case of metals, however, like sheets of iron or copper, the change in the fluorescent effect due to the diffusely reflected radiance ceases as soon as the reflecting plate becomes thick enough to be practically opaque to the direct ray.

e. The human body when in the path of the X-rays will act as a reflector. It is quite an easy matter to detect a person walking across the room in the vicinity of the slit, for as soon as a person crosses the

path of the X-rays the fluoroscope will light up. While making this particular observation I noticed that when the tube was operating especially well a faint fluorescence was still present even if no reflecting body was in front of the slit. Precautions were observed to exclude any radiance that might reach the fluoroscope directly by a sort of diffraction around the edges of the slit, but still the fluorescence in the fluoroscope persisted. There was evidently a diffuse scattering of the Röntgen radiance in the air itself. This, however, is so small that it is distinctly noticeable only when the tube operates so powerfully that a strong image of the hand on the fluorescent screen can be obtained by the radiance reflected from a pine board two inches thick and 16 inches square, placed at a distance of six inches from slit. With a good sized tube of proper vacuum and working at the temperature of highest efficiency this intensity is not at all difficult to obtain, provided, of course, that one has sufficient electric power to excite the tube.

These experiments prove beyond all reasonable doubt that the Röntgen radiance is diffusely scattered through bodies, gases not excepted. We may call it diffuse reflection, if we choose, provided that we do not imply, thereby, that we must necessarily assume an internal inter-molecular regular reflection, in order to explain the phenomenon. For if a puff of smoke be forced through a pile of wood some of it will come out pretty well scattered, although we cannot speak here of a reflection in the ordinary sense, but rather of deflection, reserving the term 'reflection' for those particular cases in which the angle of incidence is equal to the angle of deflection. It might turn out, for instance, that the X-rays are due to a circulating motion of ether and that the stream lines are deflected and diffusely scattered within the molecular interstices of ponderable substances. Appearances seem to speak more in favor of this view than in favor of a wave motion of ether.

The diffuse scattering of the Röntgen radiance by bodies placed in its path may be also described by saying that *every substance when subjected to the action of the X-rays becomes a radiator of these rays*. This statement will be more complete than the statement that a diffuse reflection takes place, if my observation should prove correct that the maximum effect in the fluoroscope is obtained when the largest surface of the body, acted upon by the Röntgen radiance, is placed parallel to the fluorescent screen. For in that case there is actually secondary radiation due to the diffuse scattering which proceeds normally to the surface of the intercepting body.

The fact that opaque bodies like metals are less effective in producing this secondary radiation leads to the conclusion that there is in these bodies an internal dissipation of the Röntgen radiance much greater than in the case of transparent dielectric substances. A properly constructed bolometer should give us much information on this point, and it is my intention to take up this subject as soon as time and facilities will permit.

These diffusion effects, which are present even in air, bring the Röntgen radiance into still closer resemblance to the principal features of the cathode rays which were studied by Professor Lenard. The difference in their behavior towards magnetic force is still to be explained. Is it not possible that this magnetic effect in air is masked by the diffuse scattering of the X-rays?

In conclusion I wish to observe that among the several theories proposed to account for the properties of the X-rays we may insert one which can be easily inferred from the somewhat neglected essay which the late Prof. v. Helmholtz wrote toward the closing days of his life. It is the essay,

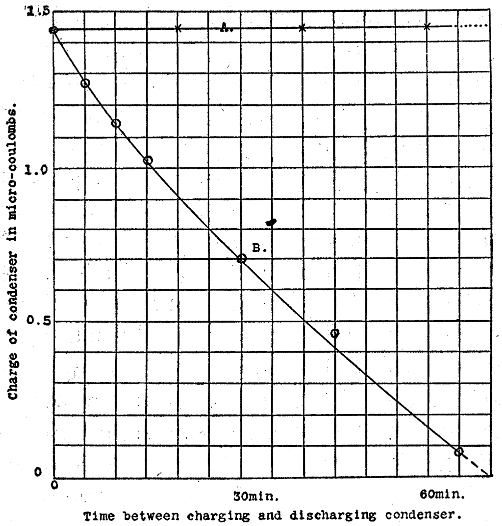
'Inferences from Maxwell's theory concerning the motion of pure ether' (Wissenschaftliche Abhandl. B. III., p. 526, Wiedem. Am. Vol. LIII., p. 135–143).

M. I. Pupin.

Columbia University, April 2, 1896.

A METHOD OF DETERMINING THE RELATIVE TRANSPARENCY OF SUBSTANCES TO THE RÖNTGEN RAYS.

The fact that the Röntgen rays have the power of dissipating the charge of a perfectly insulated electrified body was established by Professor J. J. Thompson,* and furnishes us one of the simplest methods of detecting the rays. This effect is the basis of a very simple method of making quantitative measurements of the intensity of the radiation. If we take a condenser and allow the Röntgen rays to fall upon it, we shall find that there is a very considerable diminution in its insulation resistance, and that the charge of the condenser is gradually dissipated. This is illustrated by the



curves A and B in the accompanying figure. A was obtained under the ordinary conditions. B was obtained when the Crookes tube was in action, and placed about six inches from the wooden side of condenser. The curve A was determined before, and again immediately after the determination of B. The two determinations of A were identical, showing that the effect of the Röntgen ray on the insulation disappeared with the cessation of the ray. In making these measurements a Nalder micro-farad condenser was used, the condenser being charged with a standard Clark cell. It is evident, therefore, that it is possible to compare the transparency of different substances by allowing the rays to pass through screens made of the substances and placed between a Crookes tube and the condenser and measuring the resulting leakage of the condenser.

I am now engaged in making a series of measurements, using this method and a condenser especially constructed for the purpose, and hope to give the results in a subsequent number.

It would seem that the method is capable of giving results much more quantitative in character than any that can be obtained by photographic methods.

Wm. Lispenard Robb.

Trinity College, March 25, 1896.

*London Electrician, February 7, 1896.

AN APPARATUS FOR THE STUDY OF SOUND INTENSITIES.

The study of sound intensities presents many difficulties to the physicist as well as to the psychologist; the determination of the equality of loudness of two sounds, as well as of the law of relation between the physical cause and the sensational result is perhaps the most serious one. The facts that sounds must be estimated successively and should be of a constant intensity from beginning to end further complicate the problem. The method of the falling ball has been most frequently used; it consists in dropping a ball successively from two different heights and recording the minimum difference in height necessary to

enable the observer to determine which fall gives rise to the louder sound. The objections to this method are many and obvious; it answers well enough for a demonstration, but not for exact research. A second method consists in moving an object producing a constant sound, such as a ticking watch, or a tuning fork, uniformly towards or away from the ear, and recording the minimum change in position, that enables the observer to determine whether the sound has grown louder or lower. This has advantages over the falling ball, but is far from satisfactory; and both are alike limited in the scope of their applicability. There is also an electrical apparatus, an audiometer, that is useful in determining the sensitiveness to minimal sounds, but is not so satisfactory for determining differential sensibility; the sound moreover is very artificial, difficult to listen to, and difficult to reproduce. A common defect of all the methods is the difficulty of determining by an objective test whether the sound produced by the apparatus on one occasion is really the same in intensity as in a succeeding trial.

It was in the attempt to secure a means of gradually increasing the intensity of a sound, just as the siren gradually changes the pitch, that I succeeded in devising a moderately satisfactory apparatus for this purpose. No apparatus can be regarded as completely satisfactory unless its operation depends upon a principle that clearly establishes the relation between the physical stimulus and the sensational result. Unfortunately the physicist is not as yet ready to define and measure the various factors contributing to the tones produced by the the apparatus about to be described. In the absence of such knowledge the apparatus can be proposed only as an empirical solution of certain phases of the study of sound intensities. The apparatus makes use of the principle of the singing flame. A singing flame consists of a very fine jet of gas, burning through an aperture of about one millimetre, under a long, narrow glass tube; the pitch of the resulting tone varies in an inverse sense with the size of the tube. (For details see Tyndall, *Sound*, Lecture VI.) The sound is due to the vertical vibrations of the flame, the pitch being determined by their frequency and the intensity by their amplitude. The amplitude, however, can be directly observed; the flame is first turned down until the sound just ceases to be heard, and this point is noted on a millimetre scale placed in back of the flame; when the flame is turned up to any given point the intensity of the resulting sound is clearly marked by the amplitude of the flame, as determined by the height of the flame above the zero point just described.

The other requisite of the problem is a means of delicately regulating the flow of gas and thus the intensity of the sound. This was accomplished as follows: An ordinary steam valve was taken apart and the coarse thread adjustment replaced by a fine one ($\frac{1}{40}$ inch), at the same time giving the end of the pin a delicate taper; the handle of the valve was then firmly fixed to the center of a wheel ten inches in diameter; this larger wheel was moved by the friction of a smaller wheel one inch in diameter, having at its center an index moving over a dial eight inches in diameter. In this way a movement of the index along the circumference of the dial magnified about 100 times the change in the height of the flame. The height of the flame is first determined for a few points by sighting it through a lens, and the divisions of the dial are then made accordingly.

One further difficulty remained, namely, to secure a constant pressure of gas. This was accomplished with sufficient accuracy by forcing the air out of a bell jar (fitted

with a gas cock at its neck) by immersing it in water, and then filling it with illuminating gas from the city supply. The movement of the bell jar as it descended into the water, and thus forced the gas to the flame, was carefully guided and the weight of the glass jar itself exerted a sufficient pressure. The apparatus is extremely sensitive and must be kept free from vibrations and draughts of air.

The use of the apparatus in the experiments for which it was designed is to determine the minimum change in the amplitude, the nature of which, *i. e.*, whether an increase or decrease of intensity can be detected. A sound of an agreeable intensity (and determined by a constant height of the flame) is taken as a starting point, and the subject informed that this sound will very gradually increase or decrease in loudness; he listens carefully with his head in a fixed position and answers as soon as he is confident of the direction of the change. The operator slowly moves the index in one direction or the other, takes the position when the answer is given and also the time of the experiment.

How far this apparatus will be serviceable for other methods of studying the sensibility to sound intensities is in some measure still to be determined. It may be noted, however, that it lends itself readily to determining absolute sensitiveness to sound; for one has only to note the minimum height of flame giving rise to a just audible sound with the head at a fixed distance from the apparatus. For the method of just observable difference one may have the flame sound, stop it, and sound it again with a slightly modified intensity until the difference between the two sounds becomes perceptible. For the method of right and wrong cases the same mode of use is available, except that the difference between the two sounds in any one series of experiments remains constant. By the method of the average error one should have two singing flames sounding alternately, the subject attempting to set one of them so that the sound it emits equals in intensity the standard sound. To all these applications there are as yet two objections: First, the sound does not begin immediately after the flame is allowed to play, but takes a considerable time to rise to its full intensity. The sound may be stopped instantly by suddenly lowering the flame, or placing a card at the top of the glass tube; but its inertia in starting introduces a disturbing factor. The second objection refers to the difficulty of constructing two such pieces of apparatus exactly alike, so that two flames vibrating with the same amplitude may be regarded as giving out sounds of equal intensity. Neither of these difficulties is insurmountable, and it is to be hoped that they will be solved as occasion demands.

In concluding, it may be well to indicate again that the success of the apparatus is due to the fact that the change in amplitude, and hence in intensity, can be directly observed; secondly, that the sound is fairly pure, of a definite pitch, agreeable and continuous; and thirdly, that it may be most delicately changed. All these advantages result from the use of the singing flame as a source of sound.

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HOW NATURE REGULATES THE RAINS.

WHEN American enterprise invaded with its iron cavalry the mountain regions of the West, many established theories were put to new tests and not all sustained themselves. The relations of plant life to water supply as found on the eastern half of the Continent had led our fathers to believe that the destruction of forests would invariably and inevitably result in the depletion of adjacent streams and to all consequent evils. So potent is the thick shade

which covers the ground in many parts of the Eastern States that no one imagined that conditions existed elsewhere which would produce entirely different results. The building of long lines of railroads and the opening up of mines have led to the cutting off to the very ground, of extensive tracts of timber, and the effect upon local streams has forced observing people upon the spot to the conclusion that nature has surer and wiser methods than she has been given credit for. That she has storage facilities among the mountain tops, capable of resisting the attacks of any human vandals and that the fountains of her rivers will be preserved to send down the precious floods throughout all future time, regardless of what man may say or man may do.

Any discussion of such a question among those who have looked at it from any certain standpoint will be met by the suggestion that close measurements extending over long periods of time and covering widely separated points will be necessary in order to prove anything, but the question whether snow lasts longer before or after the timber is removed can be considered without going into any of the difficult theories as to whether the fall of moisture is or is not affected by trees. In all such things a great many small considerations go to make up the great answer. Scientists point to a whole list of phenomena each one of which, by itself, would hardly have been felt, but each supporting all the rest and all coming together, produced the glacial age, and they say that when they all drop in together again at any future time, the result will be the same and miles of ice will pile up on the surface of the earth. In a small way the same is true in meteorology, and it is with an effort to give to each its due weight that I endeavor to point out some of the reasons why many close observers, after long years of study, have been led to believe that if there is any difference in the flow of streams and the size of springs before and after the trees are cut from above them, the balance is in favor of the open country.

That water which drops on shaded ground which is thickly overspread with spongy leaves and the air so near the dew point that it cannot absorb much more moisture should be held back, while that coming down on open ground should run off quickly, seems very natural, but in high mountain regions there are peculiar combinations which do much to modify the action of the law. The pine and the fir are the only trees found growing at high altitudes in any abundance, and their thin needles do not make the heavy shade when on the tree, nor the thick mat when lying on the ground, that the broad leaves of the oak, beech and maple do. Instead of forming a spongy layer five or six inches thick, they are swept about by the wind and it is not unusual to see the ground bare under the trees and all the needles lodged somewhere in drifts. Even when they lie where they fall the coating is comparatively worthless so far as retaining moisture is concerned.

On the other hand, the foliage on this class of trees being as heavy in winter as in summer, the branches catch an immense amount of the falling snow and hold it up in mid-air for both sun and air to work upon, and only those who have had experience of the absorbing power of the dry mountain air can form any idea of the loss from that source. Such as is melted falls upon that beneath, and breaking the surface sets in operation the forces which are always ready to attack such substances. The theory that the shade protects the moisture laden soil means but little in such places. The law is doubtless in force with more or less strength wherever moisture falls and plants grow, but the class of trees that thrive here require a loose, sandy soil, and are often seen growing

where there is no earth in sight at all clinging to the sides of cliffs, so bare that the roots run along on the surface entirely uncovered until they reach some crevice which they fill, and send tendrils down to draw sustenance from an unseen source. In such places the melting snow disappears quickly from the surface, and, except for their influence in keeping the soil light and porous so that the water can be absorbed readily instead of running off, it matters but little whether trees are there or not. No moisture remains on top of the ground long for shade to protect. It goes either into the air or else into the ground, and it is a well known fact that a very large portion of the water which finds its way down the steep sides of the Sierras disappears near its sources and is found again far below, either in springs, by means of artesian wells, or in the increased flow of the parent stream. Indeed, a number of very respectable rivers, not only in the mountains, but in some of the valleys, seem to owe their existence to such distant and hidden sources. If the trees have any direct power here it seems to be to draw from deep beneath the surface the moisture which has sunk into the earth and exhale gallons and gallons of it hourly. Any good sized tree has been estimated to have a capacity of forty or fifty gallon every twenty-four hours, and a forest of such trees would effect very considerable results. I should like to offer the opinion of Captain J. B. Overton, of Virginia City, Nevada, just here. He has had control of the water supply of that city for many years, and also conducted large operations in the mountains in cutting timber, wood and lumber, for the mines. His experience covers a quarter of a century and extends over several townships of land, from which his men cut the timber. He says "My experience proves to me that the cutting of the timber makes no difference in the amount of snow that falls, but that it drifts more, and for that reason lasts about as late in the summer as it would before the removal of the shade. I do not think the streams get low any sooner or afford any less water. I am of the opinion that the trees absorb from the soil quite as much water as would be evaporated by the action of the sun in the absence of shade. I know two small springs that ran for the whole year for ten years after the land was cut over, but, that since the thrifty growth of young pines have reached a height of from 15 to 25 feet and shade the ground as well, if not better, than the large trees did, have dried up about the last of August for five years past, and I can see no cause for it except that the trees are using the water. The supply of water used by my company in its operations has not decreased with the disappearance of the timber, and I do not find that the freshets are any more frequent or more violent than before the trees were cut off. The trees are coming up in a second growth much more numerously than they were before, and after sixty or seventy years about nine-tenths of them will die off and decay, leaving the timber about as it was when we first came to the country; then I think my springs will flow again. My observation teaches me that the amount of rainfall is not affected by denuding the mountain-side, but that the surface of the ground will be heated more by the sun and will therefore be drier, but that the springs and streams will be more diminished by the water used by the trees than by evaporation in their absence."

In a timber belt the snowfall is comparatively evenly distributed and by the radiation and reflection of heat from its own body each particular tree immediately sets itself to work to clear the ground around it, and long before there is a vacant foot out in the open a space will be bared for several

feet around each trunk. So long as there is no color but pure white for the sun's rays to work upon its heat is largely latent, but let a stick or straw break the surface and it will melt the snow or ice for several times its diameter on every side and stand alone in a few hours. Precisely the same is true upon a larger scale of every stump and tree in a forest. Following the reappearance of the sun after every storm the process begins, slowly or rapidly according to the temperature, clearing up large patches before that beyond shows signs of a break. This is not theory or hearsay, but actual observation covering a score of years spent in daily contact with the subject in all its phases. But it is supported by a theory also. It is a well known fact the temperature in a forest is always several degrees higher than it is on open ground under the same conditions otherwise. A series of observations were made by Cornell University several years ago, and although the belt of woodland was only half a mile long and sixteen rods wide the results were very marked. The trees were oak, maple and chestnut, with some hemlock and pines intermixed with an abundant undergrowth. The thermometers were changed and one put in another's place frequently in order to detect possible errors. The reporter sums up as follows:

"A study of the records will show that the temperature of the wooded belt is somewhat higher than that of the open field, amounting to from 2 to 4 degrees on the average; that fluctuations are less extreme and less rapid, and that gradual changes in the temperature of the field do not affect that of the belt until a day or two later."

Five different stations were kept open for several months; one thermometer being placed against the trunk of a large oak tree, near the center of the woods; one near the same tree, but not touching it; a third on a pole four feet from the ground, ten rods from the edge of the woods, and two others in the trunks of trees. A considerably warmer temperature was shown by the instrument suspended from the oak tree, but not touching it, although on several days the one out in the field was exposed to the sunshine, while the others were in the shade all the time. Of course the higher temperature would have a two-fold effect upon a snow bank. The warmer the air, the greater its capacity for holding moisture and, consequently, the greater evaporation, and at the same time its melting power would be enhanced to that extent and the snow set to running away as water. Too little weight is generally given to the fact that the rays of the sun must be broken up in order to release heat. A good example is given every spring by John Huntington, who is the owner of the toll road extending from Truckee, California, to Lake Tahoe. The snow shuts this road up very early every winter and a deposit of twenty to thirty feet is nothing unusual. As soon as possible, in the spring, Mr. Huntington sprinkles black dirt on the surface of the snow above where his road is known to be and the effect is wonderful. The layer is not heavy enough to shut out the light from striking the surface of the snow, but it is ample to release the heat rays, and there is a long depression that looks like an artificial excavation in a few hours, and days before the ground is clear on either side the stages are running on bare ground.

Trees tend to dissipate the snows in springtime also, by breaking up the steady cold winds which come down from the north at that season, almost invariably. When the current is permitted to flow on in uninterrupted sweep it retains the chill, but let it strike a forest and wind in and out among the trees for a mile or two and there will be a decided change in its temperature. It will be much better prepared to absorb moisture and also to melt the snow banks

in its changed form, as it pursues its southern journey.

But the strongest force at work to save our rivers is the drifting winds which heap up the snow in great banks, and in this the trees are a constant obstacle. There will be miniature drifts, it is true, but nothing to what there are when there is no obstruction. Outside the timber belt, where there is nothing to catch the snow as it falls and nothing to break the force of the wind, one of the most powerful and active agents in preserving the water supply of a country comes into play. By forming solid bodies of snow the most effective means of saving water for summer is reached. Across the bleak summits and down the vast canyons the wind has a well-nigh irresistible force and it not only gathers up the snow after it has ceased to come down, but it usually keeps at work all the time it is falling and carries it in whirling clouds until it strikes a cliff or a canyon set at just the right angle, and there it deposits the whole load. As long as there is any material left outside to work upon this is kept up, and there is no knowing how deep some of the big drifts get to be in the course of a long winter. As the days get warmer, the surface thaws a little and moistens the cake down a few inches, but the cold nights found all the year around at such altitudes soon transform it into ice, making a crust upon which the heat of the sun and the absorbing power of the air find it difficult to make any impression. On open ground the process is aided by the packing power of the wind, and it is not an unusual sight to see a man on horseback traveling comfortably across snow banks high enough to hide both the horse and his rider many times over if they should chance to break through. It is this which has changed the opinion of four settlers out of five along the eastern base of the Sierra Nevadas, where the timber has been cut for the Comstock mines. Over half a billion dollars in treasure have been taken from that one lode, and it is said that for every ton of ore taken out the equivalent of a cord of wood has gone in either in the shape of timber or of fuel. The whole mountain side for a distance of thirty miles has been cut over, covering the heads of such streams as Hunter's Creek, White's Canyon, Thomas Creek, Galena, Steamboat and other small rivers, which have furnished water for irrigation since 1860 to the owners of probably twenty thousand acres of land in valleys below. The concensus of opinion among this class of citizens, intelligent American farmers all of them, is that there is virtually no diminution in the supply of water that reaches them from the hills. James Mayberry had charge of men who cut over 12,000 acres in the early '70s. He is of the opinion that Hunter's Creek, with which he is most familiar, has a more certain flow and somewhat more water than before. John Wright has lived for thirty years on Steamboat Creek. It was dry in 1864, when the timber was standing, but never has been since, and has furnished water for a constantly increasing settlement. Robert Jones lives on low land and says he has had more crops killed by flooding in the ten years after the timber was cut than in the ten before it was touched. G. R. Holcomb says the supply is equally certain if not more so and attributes it entirely to the drifting of the snows. Several made answer that the water melted earlier and ran off sooner and said any one would know that, but failed to convince even themselves that they were lower than in former years.

Hon Ross Lewers, of this county, read a paper before the American Horticultural Society a few years ago in which he said: "There are certain peculiar conditions that prevail in Nevada that I think are worthy of notice. One of them is, that wherever the forest timber has been cut off, a new

growth has sprung up much thicker, and none of the young trees will start until the old ones are gone. Another is that the water supply from the mountains is greater and more permanent now than it was before the timber was cut off. The reason for this is that the wind has a more unimpeded course, and as all the snow storms come from nearly the same point in the south the snow is blown over and lodges on the north sides of the ridges where it is piled deep in drifts, and not being exposed directly to the sun's rays it melts very slowly and thus affords a more permanent supply. Spring floods are less frequent and for the same reason. I do not pretend to decide how much, if any, the presence of trees induce precipitation. They may moisten the air, but the humidity is all taken out of the ground by the roots, and I observe that the undergrowth and grass is more luxuriant since the timber was cut off."

It is hardly necessary to point out the advantage of having the snow supply heaped up in large drifts or buried deep in the canyons rather than to have it spread out, exposing large surfaces to the sun and the dry air, which in such places is almost constantly in motion, thus multiplying its capacity. In drifts the melting is almost all done at the bottom, and far into the summer a little rill will be found running away from the lower side. Good sized caves are often formed in this manner, and sometimes the top crust is so solid that the last seen of a big drift will be an arched shell of frozen snow reaching from one wall of the canyon to the other. The beautiful adaptation of the means to the end seen everywhere in nature is illustrated here. To attempt to hold back an adequate supply of water for a great region like that lying below the Sierra Nevada range in any except a solid state would be utterly useless. Nothing in a liquid form would tarry long on a heavy grade. No shade nor mat of leaves would be strong enough to overcome the law of gravitation to that extent. Nothing could detain it but a short time at the farthest, and if it were not for the vast drifts which hold the snow in an icy grasp until late in the summer, all the horrors prophesied from spring floods and summer droughts would be realized. As it is, I notice that heavy storms continue to visit the places from which the timber has been taken, but when an unfavorable season fails to bank up the drifts there is no water in the streams whether there are trees or stumps on the ground. There are places in Nevada which would give a strong support to the theory that the cutting off of timber brings frequent floods, if any had ever been there, for since the settlement of the country there have been several terrible floods which have been given the name of cloudbursts on account of the suddenness of the rise and subsidence of the water. The town of Austin, Nevada, is a sample. It has been swept several times by sudden floods, and as it lies in a narrow canyon which opens out above and spreads into quite a watershed, it is in constant danger. There never was a forest there and in early days there were no cloudbursts, but the discovery of rich mines led to the whole basin being tramped over and over constantly until the ground was as hard as a pavement. The result was that rains which formerly were taken into the soil ran down the waterways into the main canyon, which soon collected a roaring torrent and swept everything out. In so large a subject there are many things to consider and many unknown quantities to discover and weigh, but it seems to me that it is worthy of more attention than it has received. My observations, while they have extended over a long series of years, have been those of a layman and have not been such as to afford mathematical proof, even that a given

quantity of snow, say a foot, will last as long on open ground as it will among trees. As I have laid much stress upon this matter of evaporation which some may think hardly applies to snow, I will say that a considerable body has been known to disappear from our streets without making a particle of mud, leaving the ground dusty, showing that none of it melted, but that it all went directly into the air. And this will occur any time when the thermometer does not go above 32 degrees within a short time after a storm. The importance of presenting as small a surface to the action of such an air as that is very apparent, and it is in storing up the snow in heaps and packing it away in deep pockets that the economy of nature is manifested. The center of the body will not melt at any time and it requires a very warm day to get at the under side of a snow drift. The grass will be growing all around it before the ground underneath it gets warmed up sufficiently to start a stream from it, but let a tree stick its head up through the crust and it will go quickly. I have yet to see the first body of perpetual snow lying among trees. It will hardly do to say that the timber lies below the line of perpetual snow, for there are many banks which only disappear entirely once in ten years or so, when there comes a long dry summer, which have trees growing higher up on the same mountain side.

In any case I do not wish to be understood as favoring the destruction of the forests of this or any other country. I never cut down a tree in my life and never saw one fall without feeling that I had lost a friend. Whatever is proven there will always be abundant reasons for preserving extensive tracts of woodland everywhere that trees will grow, and it is time the matter became one of public concern.

R. L. FULTON.

RENO, NEVADA.

CURRENT NOTES ON ANTHROPOLOGY.

THE QUESTION OF THE CELTS.

THIS question has broken out afresh in Europe, as is the case every few years. The immediate cause was the publication of an essay, by A. Bertrand and Salomon Reinach, entitled, 'Les celtes dans les Vallees du Po et du Danube,' in which the authors claim that the proto-historic culture, the remains of which are found in the valley of the Po, is akin to that of an approximate age in the valley of the Danube, and that both were the products of the 'Celts.'

Prof. Virchow, in a lecture published in the 'Correspondenz-Blatt' of the German Anthropological Society, December, 1895, reviewed their arguments, substantially agreed with them, and further extended the area of this so-called Celtic culture.

By 'Celts' the archæologists understand a series of independent tribes who about 500—1,000 B. C. inhabited central and portions of western Europe. Their language was of that Aryan family which we now know as Celtic, represented to-day by Irish, Highland Scotch and Welsh. In stature they were tall, their skulls narrow (dolichocephalic), their complexion ruddy, eyes blue or gray, hair blonde or reddish. By the Latins they were called Celti, Galli or Galatæ, all three words from the same root *kel*, meaning violent or warlike.

The anthropologists, however, headed by Broca, apply the term 'Celts' to a small dark race in central France, and this leads to wild confusion. A long discussion, aimed to clear up the subject, by Dr. Lefevre, Dr. Collignon, Mortillet and others, has appeared in the Bull. de la Société d'Anthropologie of Paris, 1895. It is worth attentive reading by any one who desires the latest on this vexed question.

DANISH ANTIQUITIES.

PROFESSOR JAPETUS STEENSTRUP, of Copenhagen, has lately issued two memoirs of

much interest to students of Northern antiquities, both published in the 'Memoires de l'Academie Royale des Sciences de Danemark.'

One is a discussion of the remarkable so-called 'silver vase' exhumed in 1891 at Gundestrup. Upon its sides were numerous singular figures in relief, and it has generally passed as an example of old Norse work. This view is disproved by Professor Steenstrup, who shows that without doubt it is part of a series of decorations from some Buddhist temple in northern Asia. His memoir is abundantly supplied with plates and illustrations showing the identity of motives. It probably was a part of the spoils of some ancient raid which by exchange had reached the western shore of the continent.

His second memoir is another study of a similar character, bringing out the relations which in proto-historic times existed between Scandinavia and northern Asia. It is entitled 'Yak-Lungta Bracteaterne,' and contains numerous illustrations of gold bracteates from the two regions, showing the same character of design and workmanship. D. G. BRINTON.

UNIVERSITY OF PENNSYLVANIA.

SCIENTIFIC NOTES AND NEWS.

ASTRONOMY.

THE February number of the Monthly Notices of the Royal Astronomical Society, copies of which have just been received, contains the annual reports of the directors of the British observatories for the year 1895. Many of these reports are very interesting, and they show that the customary astronomical activity has not decreased. The routine meridian observations and those of comets, etc., have been carried on with the usual success. Nearly all the plates for the astrophotographic catalogue, and some of those for the chart, have been taken at the Greenwich, Cape, Oxford and Sydney observatories. The work of measurement has also made quite satisfactory progress. We quote the following from the Greenwich report:

"Towards the determination of the right ascensions and declinations of the stars the following steps have been taken: From the right ascensions and declinations given in the catalogues of the *Astronomische Gesellschaft*, 'standard coördinates' have been deduced for all stars on 72 plates which are contained in these catalogues. (By standard coördinates are meant the rectangular coördinates of the stars on the plates.) By a comparison of these with the measured coördinates, plate constants have been determined, from which the standard coördinates of other stars on the plates may be obtained by means of a linear correction, and the right ascensions and declinations deduced by a trigonometrical transformation, if desired. A full account of this, as well as the comparison of thirty overlapping plates, is given in the Monthly Notices, January, 1896."

The above shows that the reduction of the catalogue plates is well under way at Greenwich. The same is true at Oxford, and, as we mentioned in a previous issue, it is also proceeding satisfactorily at Paris and Potsdam. At the Cape considerable measuring has also been done. But the most important announcement from the Cape is as follows:

"The printing in two volumes of 'A Determination of the Solar Parallax and the Mass of the Moon from Observations of Iris, Victoria and Sappho,' is approaching completion. The part of the work referring to the meridian observations of the comparison stars is by Prof. Auwers, that of the discussion of the heliometer observations of Iris by Dr. Elkin."

We have not space to refer to the many details given in the reports of the various observatories. But they are all interesting, and will repay perusal by astronomers. The Society's medal was conferred upon Dr. S. C. Chandler, of Cambridge, Mass., as has already been announced in this journal.

THE *Astronomical Journal* of March 31st contains an article by Prof. Simon Newcomb on the 'Variation of Personal Equation with the Magnitude of the Star Observed.' This is the first attempt to make a general discussion of this rather obscure point for a large number of star

catalogues. It has been known for sometime that the right ascensions of faint stars differ systematically from those of the brighter stars, on account of a peculiar form of personal error in making the observations. Prof. Newcomb now determines the amount of this personality *per magnitude* for twelve of the principlal catalogues.

It was not possible to treat the observations of each observer separately, but each catalogue was dealt with as if it were the work of a single observer. The catalogues were compared in pairs. Sixteen such pairs were treated, and for each pair the relative variation of right ascension per magnitude was computed. The results so obtained were adjusted so as to get the variation per magnitude for each catalogue relatively to the great Paris catalogue. The latter was adopted as a standard of reference because it occurs in a majority of the pairs of catalogues treated.

The relation of the Paris catalogue to the truth could be determined by the aid of the results previously obtained by Gill, Küstner, Boss and Becker. The following remarkable result was reached:

The variation per magnitude of the right ascension averaged very nearly one-hundredth of a second of time, no matter whether the observations were made by the eye and ear method or by means of the chronograph.

H. J.

GENERAL.

WE learn from the *Botanical Gazette* that plans for the Hull Botanical Laboratory of the University of Chicago have been completed. The building, of four stories and in addition a large roof greenhouse, will include a library, lecture rooms, laboratories and private research rooms for morphology, physiology and taxonomy. As already stated in this JOURNAL, Prof. John M. Coulter, senior editor of *The Botanical Gazette*, has accepted the head professorship of botany. As, however, the building will not be completed before April, 1897, the botanical staff will not be fully organized until the following autumn. With the present issue the *Gazette* passes into the possession of the University of Chicago. The same editors will remain in charge and the general plan of the journal will be the same. The editors "wish it to be clearly understood that the *Gazette* is not to be the organ of the botanical department of any university, but that it belongs to all botanists everywhere. Its relation to the University of Chicago is simply to bring it that permanence and possibility of development which the present condition of botanical science demands."

THE annual report of the Secretary of the Geological Society of Washington states that there were held during the year 1895 fourteen meetings of the Society, with an average attendance of 35, exclusive of the meeting at which the annual address of the President, Mr. G. K. Gilbert, was given. 38 communications have been presented during the year, 29 of them being announced upon the programs of the meetings and 9 being offered in the informal half hour. The various communications were presented by 27 different members. There are now 111 active members and 38 corresponding members in the Society.

THE Fort Pitt Street Railway Company of Pittsburg has given $100,000 for a zoölogical garden at Highland Park.

THE bill reported from the Committee on Coinage, Weights and Measures of the House of Representatives adopting the metric system of weights and measures as the legal standard in the United States has been defeated in a preliminary vote, which stood 80 to 65.

PROF. RAMSAY has in preparation a book which will shortly be published by Macmillan & Co., treating the gases in atmospheric air and especially the discovery and subsequent investigation of Argon.

THE Berlin Academy of Science has elected as corresponding members, M. Poincaré, professor of mathematical physics in Paris, and Dr. G. Neumayer, director of the German *Seewarte*.

THE Director of the Lick Observatory has recently received from the Minister of Foreign Affairs of the United States of Venezuela the diploma and decoration of the order of Bolivar, the Liberator. This order was founded in 1825 by Peru and adopted in 1854 by Venezuela.

It is conferred, in this case, for services to science. Dr. Holden had previously received the decoration of Commander of the Ernestine Order of Saxony (founded in 1690) on the same grounds.

THE Presidency of the Royal College of Physicians of London, regarded as the highest honor that can be conferred on a British physician, will probably be filled by the election of Dr. Wilkes, who in the election of 1893 stood next to the ballot of Sir J. Russell Reynolds, the retiring President.

THE Committee on Agriculture of the House of Representatives has reported favorably the bill creating a special commission on highways, to consist of the Chief of Engineers of the Army, the Director of the Geological Survey, and the Chief of Road Inquiry of the Department of Agriculture. The Commission is to consider, among other things, the best methods for the scientific location of highways on the public domain; the employment of the Geological Survey in the discovery of road materials; the free testing of all road materials offered; the construction of model roads, and instruction in road-making at agricultural colleges and experimental stations.

THE admirable article by Prof. William James, of Harvard University, on 'Is Life Worth Living?' in the October *International Journal of Ethics*, has been republished in book form by S. Burns Weston, Philadelphia.

D. APPLETON & Co. announce for publication a work by Prof. John Trowbridge, of Harvard University, entitled 'What is Electricity?'

THE third International Congress of Dermatology will be held in London from August 4th to 8th inclusive, under the Presidency of Mr. Jonathan Hutchinson.

A SERIES of lectures has been arranged to be given at Berlin by professors of the University during the holidays for schoolmasters and teachers. The course will include lectures on the X-rays by Prof. Goldstein, on the nervous system by Prof. Waldeyer, on metabolism by Prof. Zunz, etc.

WE learn from the *British Medical Journal* that a committee has been formed in Berlin for the celebration of the Jenner centenary on May 14th. Among the members are Prof. Virchow, Prof. R. Koch, Prof. von Leyden, Prof. von Bergmann, Prof. Gerhard, Prof. König and others. The program includes an exhibition of portraits, medals, old and new instruments, writings, etc., bearing upon Jenner's great discovery, and also a festive gathering on the day itself, intended not only 'to honor the benefactor of the universe,' but to protest against the anti-vaccination agitation which is constantly going on.

THE Committee on Agriculture of the Massachusetts Legislature has not yet been able to come to an agreement in regard to the appropriation for the Gypsy Moth Commission. It is understood that four members of the committee favor an appropriation of $200,000, four $100,000 and three $50,000.

THE steam yatch *Blencathra* will carry an excursion party to the arctic regions next summer, visiting Iceland, Greenland and Hudson's Bay.

PROF. JAMES F. KEMP, Columbia University, has consented to become one of the editors of the *Zeitschrift fur Praktische Geologie*.

PROF. J. B. CUMMINGS, since 1856 professor of science in Westminster College, died on March 31st.

PROF. B. F. TWEED, from 1855 to 1864 professor of rhetoric and logic in Tuft's College, and later supervisor of schools in Boston, died on April 2d, at the age of eighty-five.

THE anatomist, Dr. P. C. Sappey, died on March 14th, at the age of 86. He was the author of important researches on the respiratory apparatus of birds, on the lymphatics and on other subjects, but is best known for his great work on 'Descriptive Anatomy,' which was begun in 1847 and completed in 1863.

HAVING completed his report on the asphalts and other mineral resources of the Uncompahgre Indian Reservation and vicinity in Utah, based on investigations made last fall at the instance of the Secretary of the Interior, Mr. Geo. H. Eldridge, of the U. S. Geological Survey, has resumed geologic work in Florida and neighboring States, with reference more especially to the phosphate deposits of the region.

THE fourth fascicle of Messrs. Collins, Holden and Setchell's Phycotheca Boreali-Americana, has recently been issued, containing Nos. 151 to 200 of this valuable distribution of North American algæ. It is rich in species of the genus *Batrachospermum*.

Nature states that a number of admirers of Prof. Mittag-Leffler, the founder of the *Acta Mathematica*, will shortly present him with a congratulatory address, written in four languages—German, French, Italian and English—and expressing the appreciation of mathematicians of the services he has rendered to their science. It is proposed to present him at the same time with his portrait in oils, and a subscription list has been opened to obtain funds for that purpose. Prof. Appell, 6 rue Le Verrier, Paris, will be glad to receive subscriptions.

PROF. PUTMAN states in the *Harvard Graduates' Magazine* the Peabody Museum has received from the American Antiquarian Society many important archæological and ethnological specimens, among which may be mentioned the bow of a Massachusetts Indian. This bow was taken from an Indian in Sudbury in 1665, and is, so far as can be ascertained, the only authentic Massachusetts Indian bow now extant.

SOME interesting instances of human longevity have been brought to notice of late. Alexander Freeman, now at the Sailor's Snug Harbor, on Staten Island, was born December 22, 1786, and is now 110 years of age. In the Society of the War of 1812 are enrolled 33 veterans of that war, whose average age is ninety-nine years. Fourteen are more than one hundred. William Haines, who fought with the Tennessee militia at the battle of New Orleans, at the age of twenty-six, is still living at the St. Louis Memorial Home, aged 107. Davis Parks, aged 106 years, two months, is at Fowler, Mich. Percy Dyer, 104 years, 3 months, at Belvidere, Ill. Andrew F. McKee, 104 years, at Burlington, Kansas. Four years ago there were 65 names on the veteran list.

IN 'Little Africa,' a suburb of Mobile, Ala., still live a number of native Dahomians, brought over in April, 1859, in the last cargo of slaves imported from Africa. They retain many of the traditions and customs of their native land.

IN the Sunday edition of the New York *Sun* for March 29th Mr. Jeremiah Curtin, formerly of the Bureau of Ethnology, began a series of articles on primitive folk lore collected from the Indians in California, Mexico and Guatemala. He writes first on the traditions of the Uintas, a nation formerly resident on the right bank of the Sacramento from San Francisco Bay to the foot of Mt. Shasta.

THE *Revue Scientifique*, commenting on the proposal for the appointment of a permanent director of scientific work in the United States Department of Agriculture, remarks: "Nous comprenons le désir des personnes éclairées et bien intentionnées qui mettent en avant ce projet, et nous l'approuvons sans réserves; mais nous avons des doutes sur l'issue finale des événements, et ne croyons guère à la prochaine réalisation du pays d'Utopie rêvé par Morus."

ANOTHER chapter is added to our knowledge of quadrivalent lead, by Hutchinson and Pollard, in the March Journal of the Chemical Society. They have re-examined the crystals which form when red lead is dissolved in acetic acid and find their composition to be $Pb(C_2H_3O_2)_4$ lead tetracetate. The molecular weight obtained by freezing point and boiling point methods agreed with this formula as closely as is usual with the acetates. Water at once decomposes the salt quantitatively into lead dioxid and acetic acid, with hydrochloric acid the unstable lead tetrachlorid is formed, which in the presence of sal ammoniac is precipitated as ammonium plumbi-chlorid, $(NH_4)_2PbCl_6$. Lead tetrapropionate is also described. The authors point out the close resemblance of the quadrivalent lead salts to the stannic compounds, and urge the use of the name plumbic oxid in preference to lead peroxid. (It may be questioned if, after all, the widely used name lead dioxid is not preferable to either.) H.

AS already announced in this journal, two expeditions will be sent from the United States to Japan to observe the total solar eclipse. The expedition from the Lick Observatory will be under the charge of Prof. Schaeberle, who will be accompanied by Dr. Charles Burckhalter, director of the Shabot Observatory, in Oakland, and Messrs. G. E. Shuez and Louis C.

Masten. The work will be wholly photographic in character. Prof. David P. Todd, who has charge of the Amherst expedition, has already left New York with a party consisting of Mr. and Mrs. Arthur Curtis James, of New York; Mrs. D. Todd, Chief Engineer John Pemberton, U. S. N., who goes with the permission of the Secretary of the Navy; Prof. William P. Gerrish, of Harvard, meteorologist and photographer; E. A. Thompson, of Amherst, the head mechanician, and Dr. Vanderpoel Adriance and Arthur W. Frances, of New York. The party will join the yacht 'Coronet' at San Francisco and will sail to Japan by way of Honolulu. The yatch carries a large number of instruments.

AT a postponed hearing on Vivisection before the House Committee of Judiciary of Massachusetts, the proposed legislation against vivisection was opposed by Profs. Bowditch, Theobald Smith and J. J. Putnam, of Harvard University; Prof. Hodge, of Clark University; Prof. Wilcox, of Wellesley College; Prof. Sedgwick, of the Massachusetts Institute of Technology, and others. President Eliot is reported by the Boston *Transcript* to have said that in the last twenty-five years, during which experiments in physiology had been conducted in Harvard, not a single instance of a student bringing any complaint of cruelty against the work done in the physiological laboratories had ever come to the knowledge of the corporation. There was no abuse of vivisection in Massachusetts. The men whom this bill indirectly accused of cruelty to animals were the most humane, merciful, clear-seeing men in the community, devoted, year after year, to the most humane occupation now existing in the world. Their profession showed in their faces, and he appealed to the members of the committee to know whether they thought that the men who had appeared before them could be guilty of the charge implied by the application for such legislation.

IN the first essay in his studies in the Theory of Descent, first published in 1875, Weismann discussed seasonal dimorphism in butterflies on the basis of direct experimentation and concluded that "differences of specific value can originate through the direct action of external conditions of life only;" and that "a periodically recurring change of climate is alone sufficient, in the course of a long period of time, to admit of new species arising from one another." In a recent essay on the same subject (*Neue Versuche zum Saison-Dimorphismus der Schmetterlinge*; Fischer, Jena, 1895), the details are given of fresh experiments and the whole subject is discussed anew with special reference to his constantly expanding views on the 'continuity of the germ-plasm.' The experiments are interesting and carefully recorded, but no theoretical conclusions varying much from those formerly reached are given, except in the distinction he makes between direct seasonal dimorphism and that which is adaptive, when the changes in temperature serve only to open the way to the action of natural selection.

THE third paper in Vol. VIII. of the Bulletin of the American Museum of Natural History is by Dr. J. A. Allen on *Alleged Changes of Color in the Feathers of Birds without Molting*, and is a careful review of the literature on the subject. Dr. Allen makes it plain that much of the so-called 'evidence' of change of color without molt is due to careless examination of specimens, much to a wrong interpretation of facts, and that much is pure assertion without any foundation whatever. Considerable alteration in plumage is brought about by the wearing away of the edges of feathers, slight changes result from bleaching, but while there may be a slight basis in fact for some of the speculations regarding change of color without molt, the cause, in nine cases out of ten, is demonstrably due to molt. 'Intermediate stages' are caused by the fact that a given molt does not affect all individuals of a species alike, but, owing to conditions of food, health, etc., some birds are carried to a more advanced stage than others.

AMONG the lectures to be given at the Royal Institution after Easter are the following: Prof. James Sully, of University College, London, three lectures on 'Child-study and Education;' Mr. C. Vernon Boys, three lectures on 'Ripples in Air and on Water;' Prof. T. G. Bonney, two lectures on 'The Building and Sculpture of Western Europe' (the Tyndall lectures); Prof. Dewar, three lectures on 'Recent Chemical

Progress;' Mr. W. Gowland, three lectures on 'The Art of Working Metals in Japan;' Dr. Robert Munro, two lectures on 'Lake Dwellings;' Mr. E. A. Wallis Budge, of the British Museum, two lectures on 'The Moral and Religious Literature of Ancient Egypt.' The first lecture of the Friday evening course will be by M. G. Lippmann, on 'Color Photography.'

WE learn from the London *Times* that the report of the Meteorological Council for the year ending March 31, 1895, submitted to the President and Council of the Royal Society, has just been issued as a Parliamentary paper. Of the forecasts issued at 8:30 p. m., in the year 1894–1895, the percentage of complete success was 56, of partial success 27, of partial failure 12, and of total failure 6. The average for the ten years from 1885 to 1894 was 51·2 of complete success and 30·7 of partial success. The storm warnings show a percentage of 68·5 of success and 23·5 of partial success. The warnings not justified by subsequent weather were 6 per cent. These figures show a marked improvement on those for the years from 1885 to 1893 inclusive. The hay harvest forecasts show a total percentage of 89 of complete or partial success. The Council express their regret that the experiment of exhibiting, at telegraphic stations in rural districts every afternoon, the daily weather forecasts is not to be repeated. The net expenditure of the Council in 1894–95 was £15,212 0s. 11d., as compared with £15,969, 7s. 6d. in 1893–94. The sum of £1,528 0s. 10d., was paid to the postoffice for services rendered. The income of the Council was £15,300, granted by Parliament, and £721 19s. 6d., received from various other sources.

UNIVERSITY AND EDUCATIONAL NEWS.

MRS. ELIZABETH MARY LUDLOW, the mother of the late Robert Center, has given his estate, valued at $150,000, to Columbia University for the purpose of endowing the 'Robert Center Fund for Instruction of Music.'

THE Teachers' College, New York, has received from a donor whose name is at present withheld, a gift of $250,000 to complete the present group of buildings. This will make the value of the property on Morningside Heights, adjacent to the grounds of Columbia University, about $1,000,000, and will add greatly to the facilities of the College and of Columbia University, to which it is affiliated.

MR. W. C. MCDONALD, whose gift of $500,000 to McGill University was reported in this journal last week has now given, in addition, $150,000, to be used in maintaining the engineering and physics building.

THE annual report of President Dwight, of Yale University, for the year 1895, states that gifts to the University during the year have amounted to $305,301.

THE Senate of Deans of the Catholic University of Washington has decided to establish an Institute of Technology. It is proposed to construct a special building for the purpose.

THE following instructors have been appointed in Harvard University: Charles Montague Bakewell, A. M., in philosophy; James Edwin Lough, A. M., in experimental psychology; Charles Palache, Ph. D., in mineralogy; Robert Jay Forsythe, A. B., in metallurgy and metallurgical chemistry.

BARON EÖTVÖS has been made full professor of experimental physics in the University at Buda-Pesth.

DISCUSSION AND CORRESPONDENCE.

HEREDITY AND INSTINCT (II.)*

IN the earlier paper I argued from certain psychological truths for the position that two general principles recently urged by Romanes for the Lamarckian, or 'inherited habit,' view of the origin of instinct do not really support that doctrine. These two principles are those cited by Romanes under the phrases respectively 'co-adaptation' and 'selective value.' In the case of complex instincts these two arguments really amount to one, *i. e.*, as long as we are talking about the *origin* of instinct. And the one argument is this: that partial co-adaptations in the direction of an instinct are not of selective value; hence instinct could not have arisen by gradual

*Conclusion of paper of same title in SCIENCE March 20th.

partial co-adaptive variations, but must have been acquired by intelligence and then inherited. This general position is dealt with in the earlier article.

It will be remembered, however, that the force of the refutation of the Neo-Lamarckian argument on this point depends on the assumption, made in common with him, that some degree of intelligence or imitative faculty is present before the completion of the instinct in question. To deny this is, of course, to deny the contention that instinct is 'lapsed intelligence,' or 'inherited habit.' To assume it, however, opens the way for certain farther questions, which I may now take up briefly, citing Romanes by preference as before.

I. The argument from 'selective value' has a further and very interesting application by Romanes. He uses the very fact upon which the argument in my earlier paper was based to get more support for the inheritance of habits. The fact is this, that intelligence may perform the *same acts* that instinct does. So granting, he argues, that the intelligent performance of these acts comes first in the species' history, this intelligent performance of the actions serves all the purposes of utility which are claimed for the instinctive doing of the same actions. If this be true, then variations which would secure the instinctive performance of these actions do not have selective value. and so the species would not acquire them by the operation of natural selection. By the Lamarckian theory, however, he concludes, the habits of intelligent action give rise to instincts for the performance of the same actions which are already intelligently performed, the two kinds of function existing side by side in the same creature.*

This is an ingenious turn, and raises new questions of fact. Several things come to mind in the way of comment.

First. It rests evidently on the state of things required by my earlier argument against the Neo-Lamarckian claim that co-adaptation could not have been gradually acquired by variation; the state of things which shows the intelligence preventing the 'incidence of natural selection' by supplementing partial co-adaptation. Romanes now assumes that intelligence prevents the operation of natural selection on further variations, and so rules out the origin of instinct through that agency, or, put differently, that actions which are of selective value when performed intelligently are not of selective value when performed also instinctively. But this seems in a measure to contradict the argument which is based on co-adaptations (examined in the earlier paper), *i. e.*, that instincts could not have arisen by way of partial co-adaptations at all. In other words, the argument from 'co-adaptation' asserts that the partial co-adaptations are not preserved, being useless; that from selective value asserts that they are preserved and, with the intelligence thrown in, are so useful as to be of selective value. We have seen that the latter position is probably the true one; but that the inheritance of acquired characters is then made unnecessary.

Second. Assuming the existence side by side in the same creature of the ability to do intelligently certain things that he also does instinctively, it is extraordinary that Romanes should then say that the instinctive reflexes have no utility additional to that of the intelligent performance. On the contrary, the two sorts of performance of the same action are of very different and each of extreme utility. Reflex actions are quicker, more direct, less variable, less subject to inhibition, more deep-seated organically, and so less liable to derangement. Intelligent actions—the same actions say—are, besides the points of opposition indicated, and by reason of them, more adaptable. Then there is the remarkable difference that intelligent actions are centrally stimulated, while reflex actions are peripherally stimulated. I cannot go into all these differences here; but the case may be made strong enough by citing certain divergencies between the two sorts of performance, with illustrations which show their separate utilities.

1. Reflex and instinctive actions are less subject to derangement. Emotion, injury, temporary ailment, hesitation, aboulia, lack of information, etc., may paralyze the intelligence; but instinct and reflex action may keep the creature alive in the mean time. What keeps dogs alive after extended ablations of the brain cortex?

*Op. cit., pp. 74–81.

2. Reflexes are quicker. Suppose instead of winking reflexly when a foreign body approaches the eye, I waited to see whether it was near enough to be dangerous, or even shut my eye as quickly as I could, I should join the ranks of the blind in short order.

3. Reflex actions are more deep-seated and arose genetically first. What keeps the infant alive and in touch with his environment before the voluntary fibers are developed? This genetic utility alone would seem critical enough to justify most of the genuine reflexes of the organism—supplemented, of course, by the mother!

4. Intelligent actions are centrally stimulated. This means that brain processes release the energy which goes out in movement, and that something earlier must stimulate the brain processes. This something is association in some shape between present stimulating agencies in the environment and memories, or pleasures and pains. In other words, certain central processes intervene between the outside stimulus and the release of the energies of movement. In reflexes, however, no such central influence intervenes. The stimulus in the environment passes directly—is reflected—into the motor apparatus. Hence the reflex is more direct, undeviating, invariable, sure. For example, research has recently proved that involuntary movements may be produced in a variety of normal circumstances, and in hysterical subjects, when the stimulation is too weak, or intermittent, or unimportant, to be perceived at all.

5. Experiments show that the energies of the two are not quantitatively the same. Mosso and Waller have shown that the muscles may work under direct stimulation after being quite exhausted for voluntary action, and *vice versa*. They may be exchanges of energy between the two circuits involved, which give the animal increased force in this reaction or that.

6. The intelligence could not attend to the necessary functions of life without the aid of reflexes, to say nothing of the luxuries of acquisition. So not to get the reflexes would prevent the growth of the intelligence. For example, suppose we had to walk, wink, breathe, swallow, scare away flies and mosquitoes, etc., all by voluntary attention to the details and all at the same time. While chasing flies we should forget to breathe! And when should we have a moment's time to think? In this line it is in order to cite the experiments made on 'distraction,' which show that most of the common adaptations of life can go on by reflex and sub-conscious processes while the intelligence is otherwise occupied.*

7. Attention and voluntary intermeddling with reflex and instinctive functions tends to destroy their efficiency, bringing confusion and all kinds of disturbance.

These are all simple psychological facts, and more might be added showing that instinct has its own great utility even when the intelligence may perform the same actions in its own fashion. So it remains in each case to find out this utility and measure it, before we say that it is not of selective value. I should say that reflexes are generally of supreme importance and value; and if so, then natural selection may be appealed to to account for them. So, about all that remains of this argument of Romanes is the contribution which it makes to the refutation of his other one, from co-adaptations. The assumption of intelligence disposes of both the arguments, for the intelligence supplements slight co-adaptations and so gives them selective value; but it does not keep them from getting farther selective value as instincts, reflexes, etc., by farther variation.

II. But there is another very interesting question also to be settled by fact. Romanes and others cite simple reflexes as well as complex instincts as giving illustrations of the application of the principle of 'inherited habit' or 'lapsed intelligence;' and the cases which Romanes lays great stress on are the reflex actions of man's withdrawal of the leg from irritation to the soles, and the brainless frog's balancing himself.† The Neo-Lamarckian theory requires the assumption of intelligence for all of these. I have shown that granting the intelligence, that is just the assumption which in many cases enables us to discard the Lamarckian factor. But we may ask, is the assumption itself necessary for all reflexes?

*See Binet, *Alterations of Personality*, Part II., ch. 5. (Eng. trans. announced by Appletons.)

† Passage cited.

The question is too involved for treatment here; but the assumption that intelligence is necessary in any sense which make the *conscious voluntary* performance of the action always precede the reflex performance of it is very difficult to defend. For all that we know of the brain seat of voluntary intelligence, of the use of means to ends, etc., makes such action dependent in its origin upon the presence of the great mass of organic reflex processes which go on below the cortex. Complex associative processes must be genetically (and phylogenetically) later than the simple reflex processes, which, as has been intimated above, they presuppose.

But the more liberal definition of intelligence, which makes it include all kinds of conscious processes—the assumption of intelligence being the assumption of conscious process of some kind—that is a different matter. This supposition seems to be necessary on either theory of instinct, as I have argued;* for if we do not assume it, then natural selection is inadequate, as say Romanes and Cope; but if we do assume it, then the inheritance of acquired characters is unnecessary. On this simpler definition of intelligence, however, we find certain simpler states of consciousness, of which imitation is the most prominent example, serving nature a turn in the matter of development.

And on this wider view of intelligence the difference between intelligent (*i. e.*, imitative) action and instinctive reflex action is much greater than that pointed out in detail above between voluntary and reflex action. A word to show this may be allowed me, since it makes yet stronger the case against the special argument from selective fitness, which this paper set out to examine.

The differences between imitative action and reflex or instinctive action are not just those which we have found between voluntary and reflex actions. Imitation seems to be in a sense instinctive; and in the animals it seems to be, like the instincts, peripherally initiated. But it has a farther point of differentiation from the special instincts and reflexes, in that it is what has been called a 'circular' reaction, *i. e.*, it tends to reproduce the stimulus again — the movement seen, the sound heard, etc. There is always a certain comparability or similarity, in a case of conscious imitation, between the thing imitated and the imitator's result; and the imitation is unmistakably such in proportion as this similarity is real. We may say, therefore, that consciously imitative actions are confined to those certain channels of discharge with produce results comparable with the 'copy' which is imitated.

But the special instincts and reflexes are not so. They show the greatest variety of arrangement between the stimulus and the movement which results from it—arrangements which have grown up under the law of utility. They represent therefore special utilities which direct conscious imitation in each case, by the individual creature, could not secure; while conscious imitation represents a general utility more akin to that which we have seen the voluntary intelligence subserving.

If this be so, then we have to say that conscious imitation, while it prevents the incidence of natural selection, as has been seen, and so keeps alive the creatures which have no instincts for the performance of the actions required, nevertheless does not subserve the utilities which the special instincts do, nor prevent them from having the selective value of which Romanes speaks. Accordingly, on the more general definition of intelligence, which includes in it all conscious imitation, use of maternal instruction, and that sort of thing (the vehicle of 'social heredity')—no less than on the more special definition spoken of above—we still find the principal of natural selection operative and adequate, possibly, to the production of instincts and reflexes.*

J. MARK BALDWIN.

PRINCETON, March 17, 1896.

*See my article 'Consciousness and Evolution,' examining some parts of Prof. Cope's position, in SCIENCE, August 23, '95, reprinted kindly by him in the *American Naturalist*, March, '96, with reply in the succeeding issue of the latter journal.

* This and the two preceding papers in this journal are not intended as more than preliminary statements of results thrown into the form of criticisms of particular views (*i. e.*, Romanes' and Prof. Cope's). For this reason I have not brought in reference to the general literature of the subject.

THE X-RAYS.

TO THE EDITOR OF SCIENCE: As opportunity offered experiments have been made in our laboratory with the X-rays since a few days after the appearance of Prof. Röntgen's paper. Of course, we have repeated most of the experiments that have been announced from trustworthy sources; but I recall one or two observations made here that I have not seen notice of, and take the liberty of offering the account to your journal. I use a Ruhmkorff coil with Foucault interrupter. About two ampères from accumulators, through the primary gives about six-inch spark in the secondary. For a tube I have used one of my old Crookes tubes. The one I have found to work best is pear-shaped, nine inches long, four inches in diameter at the larger end, with a flat disc cathode in the small end, set with the plane of the disc perpendicular to the length of the tube, and for anode it has a Maltese cross inserted about the middle. The cross is hinged so that it may be shaken down and thus not obstruct the cathode radiation. The tube is the one designed, in Crookes set, to show that the cathode radiation is in straight lines and will 'cast a shadow.' The first plate I exposed was with this tube, the cross of the anode being up so as to cast a shadow in the end of the tube. The plate being close to the tube, a clear shadow of the anode was cast upon it. On repeating the experiment with the sensitive plate six inches distant, there was no image of the cross on the plate, which was, instead, densely 'light struck' all over. This adds another to the quite numerous proofs that the X-rays originate at the phosphorescent surface of the glass and not at the cathode. The second observation I wish to notice is a perfectly simple and commonplace method of getting a sharp clear image by these X-rays, which refuse to be reflected or refracted. It is the use of a metal diaphragm interposed between the tube and the sensitive plate. I have found a metal plate with a circular hole one inch in diameter, placed half an inch from the tube, the tube being six inches from the sensitive plate to give very satisfactory results.*

The most interesting observation is a physiological effect of the X-rays. A month ago we were asked to undertake the location of a bullet in the head of a child that had been accidentally shot. On the 29th of February Dr. Wm. L. Dudley and I decided to make a preliminary test of photographing through the head with our rather weak apparatus before undertaking the surgical case. Accordingly Dr. Dudley, with his characteristic devotion to the cause of science, lent himself to the experiment. A plateholder containing the sensitive plate was tied to one side of his head, with a coin between the plate and his head, and the tube was set playing on the opposite side of his head. The tube was about one-half inch distant from his hair, and the exposure was one hour. The plate developed nothing; but yesterday, 21 days after the experiment, all the hair came out over the space under the X-ray discharge. The spot is now perfectly bald, being two inches in diameter. This is the size of the X-ray field close to this tube. We, and especially Dr. Dudley, shall watch with interest the ultimate effect. The skin looks perfectly healthy, and there has been no pain nor other indication of disorder. I called attention to the place before Dr. Dudley had himself noticed it, and we were both for some time at a loss to account for it, as we had no previous intimation of any effect whatever.

But this little incident may bear a suggestion. The X-rays are as yet unexplained; but the suggestion, beginning with Prof. Röntgen himself, has more than once been made that they are longitudinal rather than transverse vibrations. It is difficult to distinguish a longitudinal displacement of the ether from an electric current, as far as it goes. It is a well-known method of exterminating hair, that of sending a current to its roots by a needle. If any such quasi electric current has resulted from the X-rays the effect upon the hair might be thus accounted for. The intensity of the discharge was not sufficient to heat the tube except very

*I enclose two prints, one of a hand and one of a part of the forearm, showing the effect of a gunshot wound made thirty years ago. The print shows how the ulna, some inches of which was shot away, has attached to the radius, and also shows some half a dozen shot still in the arm. It would have been difficult to get such clear shadowgraphs of objects so large as these without a diaphragm.

slightly; and the occasional small electrostatic spark from the surface of the tube to the hair, but which was hardly noticeable, will also not account for this effect. JOHN DANIEL.

PHYSICAL LABORATORY,
VANDERBILT UNIVERSITY, March 23, 1896.

INSTINCT.

TO THE EDITOR OF SCIENCE: Having read with considerable interest the discussions under *Instinct*, and having noticed the different opinions expressed concerning the eating and drinking of the chick, I thought that perhaps my personal experiments in regard to the matter might be of interest.

About eight years ago I was desirous of studying the chick before and after hatching, and for this purpose I placed about three hundred eggs in an incubator. I shall confine myself to those that were allowed to hatch.

Those that hatched were divided into two groups, an unhealthy and a healthy group. Those in the first group were fed and given water until they became strong enough to care for themselves. Those in the second group had food and water placed so that they could get them, but they were not fed nor given water, nor were they taught how to secure food and water. No tapping on the dish or on the floor, and no putting of the bill in the food or water was practiced. They were left entirely to themselves.

By watching these chicks, I noticed that they would occasionally run over their food and water, and frequently they stumbled in them. If the beak became wet, up would go the head, and the water was swallowed. If food adhered to the beak, some would get on the tongue, and it would be swallowed. In time they seemed to recognize that the food and water were palatable by repeatedly stumbling in them and getting them on the beak, and finally they *learned* how to secure them, *i. e.*, how to pick them up. I noticed that at first they did not know how to pick up, but, after repeatedly trying, they learned how. The majority of these chicks lived and developed.

Now if we consider the attempt to pick up, from observation I conclude that it was by *instinct*; but if we consider the picking up, I conclude that it was an *acquired* characteristic.

In conclusion, I might say that at the end of the third day all of the chicks—about fifty—instinctively attempted to pick up, and that at the end of the fifth day they were able to pick up and place the food or water so that it could be swallowed. J. C. HARTZELL, JR.

ORANGEBURG, S. C., March 25, 1896.

VISUALIZATION AND RETINAL IMAGE.

A STORY which has been going the rounds of the press about a successful attempt by Mr. Engles Rogers at photographing his own retinal image of a dead child, said image being produced by visualizing effort, induces me to suggest through SCIENCE that the subject is worthy of more thorough investigation than it has yet received. What effect also hallucination has upon the retina might be determined from study of insane patients dead from hallucinatory fright, etc. In some cases of sudden death by accident there seems to be evidence of a persistence of retinal image; and it seems highly desirable that hospital surgeons should have a simple instrument for investigating such cases. An image which should represent other scenes than the surroundings at time of death might be evidence for mere visualization effecting a retinal image. HIRAM M. STANLEY.

LAKE FOREST, ILL.

NAVAL EROSION.

TO THE EDITOR OF SCIENCE: An interesting locality for obtaining some measure of the interference of navigation with the normal geological cycle is the Kennebec River, in Maine. Several summers ago, chancing upon this river, I was struck with the completeness of the phenomena of erosion produced by our steamer in disturbing the water.

This stream is an estuary for nearly forty miles from its mouth. It has numerous islands and in many places steep banks. There is a vast amount of glacial material strewn along its shore which, with the matter brought down stream, has silted the river bottom completely. I noted all along the shore that the water in advance of the steamer rose slightly on the bank, but was immediately drawn back to fill the space just occupied by the boat. At some points this recession amounted to fifteen or twenty feet, and at no place was it less than

two feet. I could hear a pronounced rattle as the material was dragged down the shore, and several boulders as big as hen's eggs were rolled three to four feet. Following the withdrawal of the water was a series of waves produced by the prow and sides of the boat. These waves, some of which were a foot high, occurred in sets of three, three more noticeable sets, followed by many smaller ones. They sorted material up to the size of a walnut.

In streams, such as this one, which form the paths of commerce for many cities, the erosion produced by the combined passage of craft of all kinds must be a not-inconsiderable factor.

G. W. TOWER.

U. S. GEOLOGICAL SURVEY, Washington, D. C.

SCIENTIFIC LITERATURE.

The Polar Hares of Eastern North America, with Descriptions of New Forms. By SAMUEL N. RHOADS. Am. Naturalist, March, 1896, pp. 234–239.

The Polar Hare of North America was separated from that of Scandinavia by Leach as long ago as 1819, since which date its specific distinctness has been admitted by nearly all mammalogists. Still, Mr. Rhoads finds it necessary to reëstablish its claim to recognition, and also to drop the time-honored name *glacialis* conferred by the naturalist Leach, who described it, and to substitute therefor the name *arcticus*, under which it was mentioned by Capt. John Ross, commander of the expedition which brought back the specimen. Capt. Ross was not a naturalist and made no claim to technical knowledge of zoölogy, but in his report on the expedition he mentioned, under the heading 'Zoölogical Memoranda,' a number of mammals and birds. Among these the Polar Hare naturally found a place. His brief account of this animal begins with the words: 'Species *Lepus arcticus*, Leach,' from which it is to be inferred that Leach, who gave him the name, at that time intended to use it. Capt. Ross stated further: "Dr. Leach thinks it [the Polar Hare of Baffin Land] to be very distinct from the common White Hare of Scotland (*Lepus albus*, Brisson) and equally so from the *Lepus variabilis*, Pallas. See Appendix, No. V."—showing that all he knew of the animal came from Leach. Leach contributed to Capt. Ross' report a chapter entitled, 'Descriptions of the New Species of Animals Discovered by His Majesty's Ship Isabella in a Voyage to the Arctic Regions' (Vol. II., pp. 169–179). Leach's name *glacialis*, followed by a Latin diagnosis and English description, occurs on page 170, while the name *arcticus*, as published by Ross, is on page 151 of the same volume.

Briefly stated, the facts seem to be these: Leach, the naturalist, discovered that the American Polar Hare is different from the European and described it under the name *arcticus*, which name he changed before the report was printed, perhaps while it was passing through the press, to *glacialis*. Capt. Ross published the name and facts communicated to him by Leach, and the sequence of chapters gave him twenty pages priority. The question is, shall the name of a new species, given by a naturalist of repute and accompanied by a proper diagnosis, be set aside because an accident of sequence brings another name a few pages earlier in the same publication. This question Mr. Rhoads answers in the affirmative. The verdict of other naturalists on the same point is of interest. A hasty examination of the literature shows that ten persons have used the name *arcticus*, while thirty-six have used the name *glacialis*, as follows:

AUTHORS WHO MENTION THE AMERICAN POLAR HARE UNDER THE NAME ARCTICUS.

Ross, 1819	Trouessart, 1880
Gray, 1843, 1867	Coues, 1884
Gerrard, 1862	Murdoch, 1885
Fitzinger, 1867	True, 1887
Allen, 1875, 1877	Rhoads, 1896

AUTHORS WHO MENTION THE AMERICAN POLAR HARE UNDER THE NAME GLACIALIS.

Leach, 1819	Gray & Ray, 1850
Sabine, 1823	Audubon & Bachman, 1854
Jameson & Scoresby, 1823	Baird, 1857
Parry, 1824	Osborn, 1859
Richardson, 1825, 1829 1836, 1839	Bernard J. Ross, 1862
Harlan, 1825	Murray, 1866
J. C. Ross, 1825, 1826	Chenu, 1867
Godman, 1826	Brown, 1868, 1875
Lesson, 1827, 1842	Dall, 1870
Hamilton Smith, 1827	Allen, 1871

Fischer, 1829	Lilljeborg, 1874
Bachman, 1837, 1839	Gill, 1876
Schinz, 1844	Rink, 1877
Wagner, 1844	Feilden, 1878
Nilsson, 1847	Greely, 1888
Luben, 1848	Brauer, 1888
Waterhouse, 1848	Merriam, 1892

If there were no other reason for choosing *glacialis* instead of *arcticus*, and wholly irrespective of the merits of the two names, *glacialis* would have to be taken if we accept the rule that in cases of names of equal pertinency, the first reviser of the group has the privilege of fixing the name. *Lepus glacialis* was used without exception by all the naturalists who published on American rabbits between 1819 and 1843, including Richardson, Godman, Lesson, Hamilton Smith, Fischer and Bachman. It is obvious, therefore, that the name *glacialis* cannot be displaced unless one of earlier date be found.

Linnæus described the Arctic-Alpine Hare of the mountains of northern Europe, under the name *Lepus timidus*, in the 10th edition of his *Systema Naturæ* (1758, p. 57), and referred to his previous description in *Fauna Suecica* (1746, No. 19, p. 8), thus fixing Scandinavia, and presumably southern Sweden, as the type locality of the species. The common large hare of Europe, although often confused with *L. timidus*, is a distinct species and was named *L. europæus* by Pallas as early as 1778. The distinctness of the two was admitted by Nilsson, Lilljeborg and others, and is recognized by Lydekker, one of the most conservative mammalogists of the present day. Notwithstanding these facts, Mr. Rhoads takes the trouble to re-restrict the type locality of *timidus* to 'Southern Sweden,' and to re-affirm the distinctness of the American animal—a point conceded by nearly all mammalogists for three-quarters of a century.

Mr. Rhoads' next effort is to divide the American Polar Hare into additional species and sub-species, as follows: *L. arcticus* [= *L. glacialis* Leach] from Baffin Land, *L. arcticus bangsii* from Newfoundland; and *L. greenlandicus* from Greenland. Instead of contrasting these with one another, or with the original *Lepus glacialis* of Leach as a standard, he crosses the seas to make his comparison with *L. timidus*. Hence, if one aspires to know how the Newfoundland and Greenland Hares differ from the typical American animal from Baffin Land, he must first ascertain how each differs from the Scandinavian *timidus*, and then, by various processes of addition and subtraction, seek to find how they differ from one another. At this point he is likely to be overwhelmed with discouragement, for Mr. Rhoads does not always describe the same parts or structures in the forms he names as new. Thus, we are told that, in *L. timidus* "the radius of the arc described by the incisors is one-eighth ($\frac{12}{100}$) of the basilar length of the skull," and in *L. greenlandicus* the same radius 'is one-fifth ($\frac{20}{100}$) the basilar length,' but in *arcticus* and *bangsii* the arcs of the incisors are not described at all, leaving the student of the geometry of Leporine teeth in abject despair.

After a somewhat exhausting study of Mr. Rhoads' paper, the only tangible difference I am able to find between the Newfoundland and Baffin Land Hares is that the latter turns gray in summer, while the former turns only partly gray. This sets one to wondering if Mr. Rhoads will next separate weasels that turn white in winter from specimens of the same species that remain brown the year round.

At the close of his paper Mr. Rhoads states that he "is now preparing a more comprehensive revision, with illustrations, of the New World representatives of the *Lepus timidus* group." Let us earnestly hope that he will make it sufficiently comprehensive to tell how the component parts of the American Polar Hare differ from one another. C. H. M.

North American Birds. By H. NEHRLING. 4°, part XIII., pp. 47, pls. 2. March, 1896. Geo. Brumder, Milwaukee.

The 13th part of Nehrling's well-known work has just come to hand. It treats of the Cardinals, Rose-breasted and Blue Grosbeaks, Indigo, Lazuli and Painted Buntings, Grassquits, the Dicksissel, Lark Bunting or White-winged Blackbird, and Bobolink. The text maintains the high standard of the earlier numbers, but the two colored plates, both of which are of the 'mixed' kind, are cheaply printed and decidedly inferior.

An unusually large proportion of the birds whose life histories make up the present part are species with which Mr. Nehrling is personally familiar; as a result most of the biographies are original and more than ordinarily interesting. Mr. Nehrling not only loves birds, but he has a keen ear for the harmonies of nature. "The Bobolink," he says, "never sings before sunrise. It begins its sweet music when the more earnest and solemn melody of the Robin, which was heard from earliest daybreak, is almost at its close. Nature seems to have ordained that the serious part of her musical entertainment in the morning hours should be heard first, and that the lively and merry strains should follow them. In the evening this order is reversed, and after the comedy is concluded nature lulls us to repose by the mellow notes of the Vesper Sparrow and the pensive and still more melodious strains of the solitary Thrush." C. H. M.

The Book of Antelopes. By P. L. SCLATER and OLDFIELD THOMAS. With colored plates by WOLF and SMIT. 4°. London, R. H. Porter, 1895–96.

Since the notice of parts I. and II. of this admirable work (SCIENCE, April 5, 1895, p. 389) the first volume has been completed and one part of the second has appeared. Vol. I. contains 220 pages and twenty-four handsomely colored plates, besides numerous useful figures in the text.

Parts III. and IV. treat of the duikers (genus *Cephalophus*), and part IV., which completes the first volume, closes with an account of the four-horned antelope (*Tetraceros quadricornis*). The duikers, unlike most of the antelopes, live in brush and forests. They inhabit Africa south of the Sahara, and most of the species are restricted to West Africa. Twenty species are recognized, ranging in size 'from that of a small donkey down to that of a hare.' As a rule they are handsomely colored, though most of them lack the striking and, in some cases, startling recognition markings that characterize some of the other groups. A few of the species, however, as the banded duiker (*C. doriæ*) and the yellow-backed duiker (*C. sylvicultrix*), are conspicuously marked.

Part V., comprising ninety-two pages and six colored plates, takes up the African subfamily Neotraginæ and treats of the klipspringer (*Oreotragus*), the oribis (*Ouretria*), the grysbok and steinboks (*Raphicerus*), the Zanzibar and Livingstone's antelopes (*Nesotragus*), the royal antelope (*Neotragus*) and the dik-diks (*Madoqua*).

The book of Antelopes is a timely work and it is matter for congratulation that the colored plates prepared under the supervision of the late Sir Victor Brook more than twenty years ago are finally given to the public accompanied by such authoritative letter press. If the distinguished authors have erred in the treatment of certain species it is on the side of conservatism, and it must be admitted that they have enjoyed unsurpassed opportunities for the study of the living animals at the Zoölogical Society's Gardens, of which the senior author has had charge for nearly forty years, and for the study of skins and skulls in the rich mammal collection of the British Museum, of which the junior author has long been curator.

Still, one is filled with regret at the large number of species unrepresented, or at most imperfectly represented, in museums, and it is sad to feel that many species are on the road to rapid extinction. Before it is too late sportsmen as well as naturalists should spare no pains to secure specimens of the rarer kinds and see that they reach some of the larger museums, where their permanent preservation will be guaranteed. C. H. M.

Chemistry for Engineers and Manufacturers. By BERTRAM BLOUNT, F. I. C., F. C. S. and A. G. BLOXAM, F. I. C., F. C. S. *Vol. I.—Chemistry of Engineering, Building and Metallurgy.* Philadelphia, J. B. Lippincott Co. London, Charles Griffin & Co., L't'd. 1896. 8vo, 244 pp., Illust. $3.50.

This is the first volume of a small and concise work on Chemical Technology, which is especially intended for engineers, architects, builders and factory superintendents, as well as students of chemical technology. It is intended primarily for those whose knowledge of chemical theories and processes is limited, but so skilfully is the subject-matter presented that even trained chemists and expert engineers may find the

book helpful. All descriptions of processes and apparatus are necessarily much condensed, matters of detail being relegated to the larger handbooks and monographs on special subjects, which, in the opinion of the reviewer, is their proper place. But the addition of references to the larger and special works, either as footnotes or otherwise, would have materially increased the value of the book without altering its character as an elementary work.

The present volume consists of two parts, the first being devoted to a general introduction and Part II. to Metallurgy.

The four introductory chapters are each given to a special topic. Chapter I., 'The Chemistry of Materials of Construction,' treats of the properties of stone, brick and concrete, roofing materials, the structural metals, and the strength, permanency and preservation of these substances. Chapter II. deals with 'The Chemistry of the Sources of Energy,' viz.: solid, liquid and gaseous fuels, electrical heating, measurement of temperature, direct conversion of chemical into electrical energy and the natural forms of kinetic energy. 'The Chemistry of Steam Raising' is the title of the third chapter, which has for its subjects, water and the methods of purifying and softening it for use in boilers. 'The Chemistry of Lubricants and Lubrication' is briefly disposed of in some seven pages, forming the fourth chapter.

Part II., comprising about one-half of the book, is a fairly complete though condensed presentation of the subject of Metallurgy in all its branches. The commercially important metals, some nineteen in number, are here included, their chief ores described and the processes of their extraction set forth in a brief and readable manner. Many of the important appliances and parts of smelting and refining plants are illustrated by cuts. Numerous tables of analyses of ores and of finished products are scattered through the text. In these days of popular interest in mining and metallurgical schemes, it would seem that this section should lend the book an attraction to many persons in commercial life, though they may have little or no scientific education. The facts are so clearly and tersely stated and illustrations are so frequent that any one of average intellect, though not a chemist or engineer, should have no difficulty in understanding the work. Technical terms and chemical symbols are frequently used, it is true, but in the case of the latter the common names of the substances are also stated, hence no confusion need result.

But it is to the teacher of chemistry and metallurgy, having to deal with young students, where an elementary treatise, short and compact in its nature is desired, that this book will be most welcome. Here are found the essential facts without those mystifying details which often become magnified to undue proportions in the mind of the student.

A very complete index, free from mistakes or misprints, closes the volume.

If the second volume, covering the field of manufacturing chemistry, be as well done as this, a valuable addition will have been made to the mass of chemical literature.

FRANK H. THORP.

The Chemistry of Pottery. By KARL LANGENBECK. Easton, Pa., Chemical Publishing Co. 1896. 12 mo., pp. 197.

In this little book the author has collected and systematically arranged some of the results of an extended experience in the manufacture of pottery and tiles. The chemical bearing of each subject in its relation to the object desired is made the chief element of the work. Analyses of the materials are taken as the basis on which to calculate rational formulæ for the production of certain results.

The book is divided into fifteen chapters, each treating of a separate subject, a few of which may be mentioned. In Chapter I, Analysis of Materials and Products, and in Chapter II., Physical and Empirical Tests, are explained. The subject of Chapter III. is Pyrometry, a matter of great interest to the pottery maker, since the success of his work depends, in great measure, on the proper heat in his furnace. Estimation of the temperature becomes a matter of experience with the burner, who often acquires much skill in producing some one kind of ware in a given furnace. But if called upon to burn other ware than that to which he is accustomed, or to use different fuel, or a kiln of different construction, failure may be the result. The author

recommends the use of the 'Normal Pyrometric Cones,' invented by Dr. Seger, as affording a safe and simple method of controlling the temperature of the kiln. He considers it quite possible to prepare cones from our domestic materials, fully as reliable as those now made in Germany.

In Chapter V. that subject often so troublesome to pottery makers—Glazes, their requirements and composition—is presented. The various kinds of Ware, Bricks and Terra Cotta comprise the succeeding chapters up to the fourteenth, on Refractory Materials, in which the preparation of fire clays for use in kiln building and for "saggars," is fully explained. Sixteen pages on Burning the Ware, in which the requisites of this important part of pottery making are interestingly detailed, form the final chapter. A convenient index follows.

A few more illustrations or diagrams in the body of the work would have given it added interest for the majority of chemists who have only a superficial knowledge of the processes of pottery making. FRANK H. THORP.

SCIENTIFIC JOURNALS.

JOURNAL OF GEOLOGY, FEBRUARY–MARCH.

Kame Areas in Western New York South of Irondequoit and Sodus Bays: By H. L. FAIRCHILD. The purpose of the paper is to describe certain massive deposits of sand and gravel apparently formed by the glacial drainage. These bays are the extreme points in the great landward curve in the south shore of Lake Ontario, and are thought to have greatly influenced the drainage of the region during the recession of the ice. Four Kame areas are described—Irondequoit, Victor, Mendon and Junius. The author finds these areas alike in the following particulars: (a) they are located in the basin of Lake Warren; (b) they have an overwash or silt plain to the southward; (c) they lie in the midst of drumloid ridges which antedate the kame deposits; (d) only one has any clear connection with an extended frontal moraine. He thinks the causation is complex, including rapid ice retreat, action of lake waters to prevent great local accumulations of morainic till and heavy glacial drainage.

A Pre-Tertiary Nepheline-Bearing Rock: By F. BASCOM. The rock in question is a glacial boulder found in the vicinity of Columbus, Ohio. There was a single specimen about a foot and one-half in diameter, but it is of a type so rare as to justify in the mind of the author a particular mention. She inclines to the opinion that it belongs to the nepheline syenite porphyry group. The source is not known, but is presumed to be the area north of Lake Huron, and if so the boulder is from a Cambrian horizon or lower. In any case it is a pre-Tertiary dike or surface volcanic resembling the modern type.

Remarks on Petalodus Alleghaniensis (Leidy): By CHAS. R. EASTMAN. In a previous issue of the journal Dr. Hay described a specimen of Selachian tooth from the Carboniferous of Illinois. For the form he proposed the name Petalodus Securiger. In the present paper the author dissents from this view and gives reasons why the new name should not be accepted. His opinion is that the form belongs to P. Alleghaniensis.

Patalocrinus Mirabilis (N. sp.) *and a New American Fauna:* By S. WELLER and MRS. A. D. DAVIDSON. The fossils here described were collected by the junior author in Jones county, Iowa. Goniophyllum pyramidale and the species of Crotalocrinus have long been known in the Gothland limestone of Sweden. In this Iowa Silurian fauna, species of Goniophyllum are found indistinguishable from those of Gothland, with a crinoid whose nearest ally is Crotalocrinus. The crinoid, which is a new one, is carefully described and figured by the senior author, who finds an explanation of the similarity between the Gothland and Iowa faunas in a migration along a supposed shore line, joining the east American and British regions during Niagara time.

On the Nature of Igneous Intrusions: By ISRAEL C. RUSSELL. In a previous paper the author described some hills in the Black Hills region, which illustrated a little known phase of igneous intrusion. He now discusses igneous intrusion in the light of his large experience in many localities. Of these he finds several classes—intruded sheets like those of the Newark which, when widely extended are of easily fusible rock and relatively surperficial, lacco-

lites like the well-known Henry mountains, plutonic plugs of which there are several examples in the vicinity of the Black Hills, and deeply-seated intrusions of a viscous magma which raised vast domes of sedimentary rock with the floor of metamorphic rock on which they rested as the whole Black Hills dome, Big Horn and Park mountains. As to the cause of these uplifts, nothing less than the force exerted by a cooling globe is thought to be adequate. That they took place very slowly is inferred from the fact that fracture did not result from the bending of thousands of feet of strata. That these domes are in the interior of the continent rather than near the coast is because here the crust is relatively light and strata are horizontal, hence pressure on the plastic interior due to contraction of crust or to transfer of material on the surface would be most likely to produce domes.

Deformation of Rocks: By C. R. VAN HISE. This is the first of a series of papers on the same subject to be published in the *Journal* as 'Studies for Students.' The author divides the outer part of the earth into three zones: (1) An upper zone of fracture; (2) a middle zone of fracture and plasticity; (3) a lower zone of plasticity. Rocks under less weight than their ultimate strength when rapidly deformed are in the zone of fracture. The maximum depth at which fracture can take place is thought to be 10,000 meters. Rocks below this are in the region of plasticity and flowage. Since flowage is necessary to folding, closely folded strata were generally buried beneath other strata. The boundary between the zone of fracture and that of flowage is at different depths for two rocks of different strength, also for the same rock under different conditions of stress, hence there is a zone of combined fracture and flowage. This is thick and of prime importance. In heterogeneous strata in this zone, irregular fracturing, brecciation, jointing, faulting, folding, and development of secondary structures, may occur together in a most complex manner. Between the three zones there are many gradations.

Chas. R. Keyes contributes a careful and appreciative review of Wachsmuth and Springer's new book, *North American Fossil Crinoidea Camerata.* Several reviews and authors' abstracts of current geological literature follow.

SOCIETIES AND ACADEMIES.

GEOLOGICAL CONFERENCE OF HARVARD UNIVERSITY, MARCH 10, 1896.

An elementary presentation of the tides: BY W. M. DAVIS.

The object of this communication is to show how the tides may be treated in an essentially scientific manner in an elementary collegiate course on physiography. The facts are presented by means of tracings from selected automatic records of tide gauges in the Coast Survey office, for stations in mid-ocean (Honolulu), Pacific coast (Port Townsend, Wash.), Atlantic coast (Boston), and in estuaries (Delaware at Philadelphia, and lower Seine, the latter from French records). Mean interval of tides, and systematic variation of interval and of range are numerically determined from these records by the students in laboratory exercises. The agreement of the mean interval with half a lunar day suggests that the moon and the tides may be related in some way as cause and effect. Inquiry is then made as to the manner in which the moon could cause periodic oscillations of the ocean.

The dimensions, distance and movements of the earth and moon being given, the deforming forces due to lunar attraction, situated as it were on a shell enclosing the earth, are worked out quantitatively in terms of gravity, according to the law of gravitation. A tide opposite to the direct lunar tide, often regarded as an obscure part of the problem, is seen to be as essential a consequence of the theory as the direct tide itself. The first simple supposition of a moon moving in a circular orbit in the plane of the earth's equator is afterwards changed to the actual condition of the moon moving in an orbit of considerable eccentricity and in a plane oblique to the equator; thus introducing expectations of various systematic inequalities in tidal intervals and ranges. The essential features of diurnal inequality are simply illustrated as a necessary consequence of theory by means of a 'tidal globe,' rigged with appropriate circles for high and low tides. Solar tidal forces and

their combinations with lunar forces are easily calculated to a sufficient degree of detail.

Although the forces available for the deformation of the ocean are so small that the student may at first doubt their sufficiency as a cause of the observed tides, his doubts vanish when the consequences of the theory are systematically confronted with the generalized results of observation, and the extraordinary agreements of the two are discovered. Although a fairly complete record of facts may be made by the average college student in the early laboratory exercises, it is nearly always the case that some classes of facts will escape his first scrutiny of the tidal curves and will be revealed only when attention is called to them by the expectations of theory. Due attention is thus paid to the different kinds of verification of theory. The final acceptance of the theory becomes a logical necessity, independent of the will, even though certain features of the tides, especially of the Atlantic tides, remain beyond the reach of the elementary discussion here attempted.

The treatment of the open-ocean tide and the onshore tide, as comparable to offshore swell and on-shore surf, suffices to explain various facts as to age and range; and the treatment of the on-shore tide as a wave accounts for the peculiar relations often observed between flood and ebb currents and high and low water. It is on the basis of work of this kind that the claim is made of the essentially scientific quality of physiography. Although other divisions of the subject may not be dealt with mathematically, they all contain the logically successive phases of observed and generalized facts, postulated general principles, provisional hypotheses, consequences or expectations deduced from the hypotheses, comparison of the consequences with the facts, and final evaluation of the knowledge gained. Lunar gravity is the main force causing the tidal changes of the sea; terrestrial gravity is the main force causing the slower physiographic changes of the land.

Tidal Scour: By F. P. GULLIVER.

The speaker considered the forms produced by the tides upon flat coasts and pointed out that it is wholly a question of ratios that determines the form in any given locality. He did not agree with Mr. Shelford that deltas are produced only in tideless seas,* for there are weak tides even in the Gulf of Mexico, where the Mississippi mouths, and in the Mediterranean, where the Nile and Tiber deltas are found, while the Ganges produces its delta in the face of seventeen-foot tides. If the river is relatively stronger than the tides and other sea forces it will build forward a delta.

It is also largely a question of ratios between the on- and offshore action and the alongshore action which determines the production of broken or continuous shore lines. Where there is a broad area of marshes and flats, upon which the water lies at high tide, and then during the ebb scours runways beneath the level of the flats, it is inferred that the tidal action is the process which determines the shore forms. Off steeper coasts less tidal action is indicated. Where the shoreline is prevailingly longitudinal a ratio in favor of alongshore action is inferred.

A graded series of shore forms was shown, from that in which the pure tidal on- and offshore action is indicated to that in which the alongshore action seems to be dominant. The type of the tidal action was on the west coast of Florida, where the tides are weak, but indications of alongshore action are absent, therefore the ratio is greatly in favor of the tides. The runways are of the indefinite consequent or autogentic type of drainage, and the shoreline is minutely irregular without deep indentations. The salt marsh grades into the tidal flat.

The type of the dominant alongshore action was taken from the Texas coast. An offshore bar here forms a long gently swinging curve extending for 102 miles unbroken by a single tidal inlet. This bar appears to have an outline dominated by alongshore action.

Along the coasts of the world various combinations of different absolute values of these two actions may be seen in varying ratio. Where the values are larger the forms have greater vertical measure, as in South Carolina and in the Schleswig-Holstein region. The following series of maps was shown, illustrating the progressive change in ratios between the

* Min. Proc. Inst. Civ. Engin., LXXXII., 1885, 2–68.

tidal on- and offshore and the alongshore actions:

I. West coast of Florida (Coast Survey, 180, 181).

II. West coast of Schleswig-Holstein (Topographical map of the German Empire, 1:100,-000, 5, 11, 20, 21, 35, 36, 37, 55, 56, 79, 80, 109, 110,111).

III. Georgia-South Carolina coast (Coast Survey, 152, 153, 154, 155, 156).

IV. North Carolina and New Jersey coasts (Coast Survey, 148, 149; 123).

V. New Jersey, Virginia, North Carolina coasts (Coast Survey, 122; 138; 145, 146, 147).

VI. Texas coast (Coast Survey, 210, 211, 212).

MARCH 17, 1896.

1. *Exhibition of New Lantern Slides*, by J. B. WOODWORTH.
2. *Note on Penning's Field Geology*, 2d. edition, reissue of 1894, by T. A. JAGGAR, JR.

This book (published by Bailliere, Tindall and Cox, London) and A. Geikie's 'Outlines of Field Geology' (Macmillan, 1891) are the only books known to the writer which purport to deal with practical field methods of geology. Geikie's book is more popular in style, more elementary and more comprehensive; his chapter on the schistose rocks is excellent, while Penning does not even mention them. Penning's book, on the other hand, contains many useful tables, rules for finding true dip, tracing boundary lines and faults, levelling etc. The directions for note taking do not include mention of the coördinate method of designating points on the note-book map, nor is the use of the plane-table mentioned; in these and other respects the book is not up to date for the American geologist, but on the whole the part which deals with geological surveying, sections and levelling contains much that is useful. The part devoted to paleontology by Jukes-Browne contains many useful hints for the collector, and tables of fossils that are of course intended for use in British fields. Part V. is suggestive, dealing with some difficulties likely to be encountered by the student in the field, notes on water supply, springs and wells; stress is laid on the great importance of the study of physical features in connection with geological structure. The weakest chapter in the book is that devoted to lithology, which gives elaborate and antiquated tables of physical tests for minerals, rocks and ores, but does not touch on the difficulties likely to beset the student in the field. Mr. Penning believes "it should be unnecessary to insist upon what all geological text-books so strongly recommends, that an acquaintance with the appearance and characteristics of all ordinary rocks and minerals should be formed by careful study of cabinet specimens." He believes that "tests applied in their proper order," according to his tables, "will go far enough to arrive at an accurate solution." Rutley's 'Study of Rocks' (1879) is quoted as 'an important work, recently published,' while in the lithological bibliography no mention is made of such books as Teall's 'British Petrography' or the English translation of Rosenbusch.

T. A. JAGGAR, JR.,
Recording Secretary.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON.

THE 450th meeting was held on March 14, 1896. The paper of the evening was read by Hon. Carroll D. Wright, Commissioner of Labor. on 'The Factory System as an Element in Civilization,' showing that the factory elevates the low class of persons which it employs by compelling them to think more and be more orderly and careful than they otherwise would.

BERNARD R. GREEN,
Secretary.

THE TORREY BOTANICAL CLUB, MARCH 25, 1896.

IN the absence of the President the chair was occupied by Dr. T. H. Allen, first Vice-President, and there were present 39 persons.

Two new members were elected, and W. A. Bastedo appointed to act as Secretary during the absence of Dr. Rusby in South America.

As the summer season is now rapidly approaching, a 'Field Committee,' with Dr. N. L. Britton as chairman, was appointed to arrange for the weekly outings of the club.

The announced paper on Azaleas was postponed owing to the unavoidable detention of Mr. H. A. Siebrecht in the Island of Trinidad.

A new fascicle of the 'Distribution of North American Algæ,' by Collins, Holder and Set-

chell was shown and commended by Dr. Britton. Also a sedge *Reimaria maritima*, only lately found in Florida at Lake Worth, but having a wide distribution elsewhere.

The announced paper for the meeting was read by Miss Alexandrina Taylor, entitled 'A comparative Study of the superficial Periderm in a number of species of Salix,' and was well illustrated by diagrams. In most text-books the work of Sanio is taken as authority on the development of superficial periderm. From the large number of species of the genus Salix, he selected one as a type. The many variations from this type pointed to the possibility that, by extending the study over a greater number of species than those studied by Sanio, one might be found which might more justly be called the type of the genus. This was the object of the above study.

W. A. BASTEDO,
Recording Secretary pro tem.

WEST VIRGINIA ACADEMY OF SCIENCE.

THE fifteenth regular meeting of the Academy, which was also the first annual session of the organization, was held at Morgantown, March 24, 1896.

The following officers were reëlected:

President, Dr. A. D. Hopkins; Vice-President, Prof. Thos. C. Miller; Secretary and Treasurer, Mr. W. Earl Rumsey; Corresponding Secretary, Prof. B. H. Hite.

The President, in referring to the history and first year's work of the Academy, stated that the Academy was organized on February 25, 1895, with sixteen active members and twelve associate members, representing chemistry, physics, geology, biology, entomology, mechanical and civil engineering, zoölogy, medicine, agriculture, horticulture and general science.

Fourteen regular sessions of the Academy have been held, twenty-eight communications have been presented, and three important resolutions have been passed. The communications referred to the following subjects and branches of science:

Chemistry, 1; psychology, 3; electricity, 2; geology, 1; horticulture, 2; bibliography, 2; agriculture, 2; entomology, 2; mechanical engineering, 3; ornithology, 2; general science, 1; anthropology, 1; botany, 1; civil engineering, 1; hydrography, 2; forestry, 2.

The resolutions were with reference to the publication of topographic maps, waterways and forest preservation.

The only communication presented at this meeting besides the President's remarks was by Prof. L. C. Corbett, who announced the completion and successful test of an improved *auxanometer*, which was exhibited at work. In explanation Prof. Corbett stated that the chief features of the machine are that all parts of the instrument are mounted upon a rigid base; the usual system of proportionate pulleys has been replaced by a simple lever of the first type, *i. e.*, where the fulcrum is between the power and the weight. The record is made in ink upon a paper-bound cylinder. The rate of the cylinder is retarded to a single revolution in 24 hours. The record of each day, therefore, appears as a platted curve rather than in the form of a spiral, as is the case with recording drums making a revolution each hour. The mode of attaching the *auxanometer* to the plant has been improved upon by substituting wooden forceps with relatively broad faces for the usual bent pin; this is again connected with the recording arm of the instrument by a fine wire instead of the usual cord. In this way the objectionable features of the system of weighted cords and pulleys are overcome.

W. EARL RUMSEY,
Secretary.

NEW BOOKS.

A Compendium of General Botany: MAX WESTERMAIER, translated by ALBERT SCHNEIDER. New York, John Wiley & Sons. Pp. x+299.

Natural History of Selborne: GILBERT WHITE, with an Introduction by EDWARD S. MORSE. Boston and London, Ginn & Co., 1896. Pp. xii+251.

The Psychology of Attention: TH. RIBOT, third revised edition. Chicago and London, Open Court Publishing Co. 1896. Pp. xii+120.

An Examination of Weismannism: GEORGE JOHN ROMANES. Chicago and London, The Open Court Publishing Co. Pp. ix+221. 35 cts.